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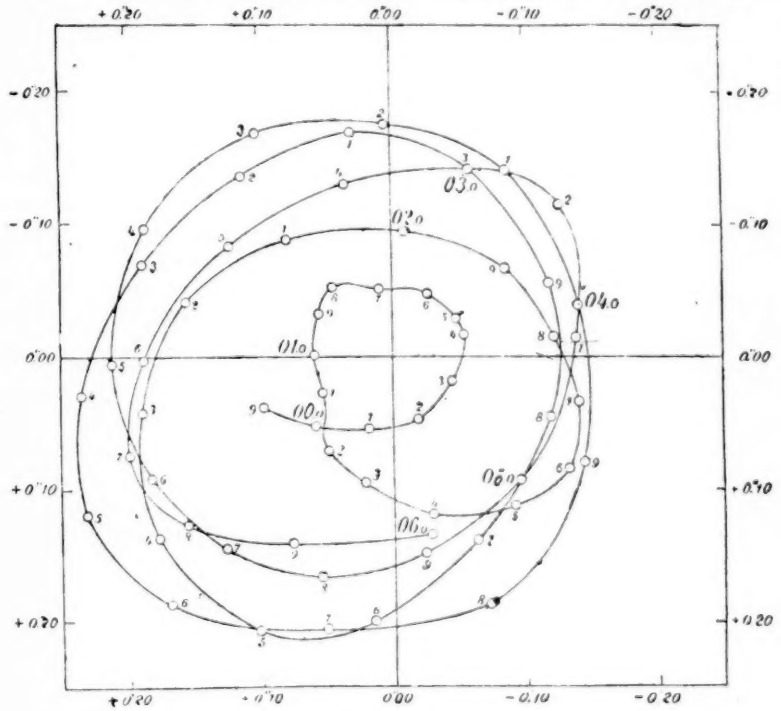
MOTION OF THE NORTH POLE OF THE EARTH ON ITS SURFACE.

W. W. PAYNE.

FOR POPULAR ASTRONOMY.

About twenty-two years ago observations were made of very great precision that revealed the fact that the north pole of the axis of the Earth was not a stationary point on its surface, but



that it must change in its position as much as a large fraction of one second in arc due to some unknown cause. It was then

thought that the Earth's terrestrial pole was in motion, in a curve whose diameter must be nearly sixty feet. If this were true, it would follow that there would be a variation of the latitude of places on the surface of the Earth proportionate to the motion of the terrestrial pole, because the terrestrial equator is located ninety degrees from the poles of the Earth's rotation.

This question of the change of latitude was thought to be so important that it was studied very thoroughly by Dr. S. C. Chandler of Cambridge Mass., and a solution of it was reached in 1891 which revealed some very puzzling things. The analytic results were put in graphic form, so that those unacquainted with the methods of the higher mathematics used could readily understand the meaning of the results. By consulting the figure given below, any reader will comprehend the details of the description we give.

This curious curve is made up of two motions; an annual elliptical motion around the axis of the Earth's figure as a center with a greater radius of about fourteen feet and a lesser one of about four feet, and another circular one of a period of 428 days with a radius of about fifteen feet. It is an important fact that both motions are from west to east. The illustration herewith given is from *A. N.* No. 4121, by Professor Th. Albrecht who has given much attention to the study of this problem, and who has written several valuable papers upon it. This last published cut by him shows the position of the pole brought down to the present year from 1899.9. During the last ten years the computers have given us quite smooth curves. They are irregular in curvature, but the motion of the pole in any of those years has not been so abrupt, and violently changeful as those mapped in the years 1894, 5, 6 and 7. It would be interesting to know why those years should give results that are so noticeably different.

That there should be an oscillation of the pole on the spheroid, because of a motion around the axis of figure is not surprising when one thinks of the make-up of the Earth as a whole. Many people think of the Earth as a rigid body throughout its entire mass. Others say it has an external crust of about twenty-eight miles in thickness and within is a molten mass of lava, and still others think the interior of the Earth, in whatever form its matter may be, must still be more rigid than steel. This latter view is that of the physicist, and if he is right, he easily provides for all earthquake phenomena in channels and great lakes of lava located in the crust at no very great depths below the surface.

It is a problem of great interest to account satisfactorily for

the two periods named above that the mathematicians have found in the study of the motion of the terrestrial pole. Many things may be thought of as having an influence on this motion; but to go far enough with these varied suppositions to make them definitely explain the results already found has not yet been accomplished. Euler in the beginning of the eighteenth century showed that the period of the oscillation of the axis of the Earth, if it does so change, would be 305 days. He obtained that result on the hypothesis that the Earth rotates as a rigid body. Other astronomers have tried to observe the oscillation, but have failed to do so, in earlier times, probably because attention was not distinctly called to the cause of known variation of results which may have been supposed to be accidental.

We ourselves remember of making a series of observations on forty pairs of stars, by the Talcott method, with a good zenith telescope, extending over a period of four weeks, for the purpose of determining the latitude of the Observatory here. In the results we found a change of value, as the observations were continued, that we could not account for on any reasonable known hypothesis, for the greatest care had been exercised in making the observations and the results were then believed to be free from all ordinary errors.

It seems to us, now, (though this point has not been proved), that the difficulty may have been the change of latitude that was going on, for the variation of results was progressive, as it should have been during the time when the terrestrial pole was moving quite directly northward, as shown in the diagram, during the years 1903-5. It seems to us that observations by the zenith telescope would be very useful in helping to settle some of these interesting questions. We are glad to know that the study of this question has been considered important enough to have observatories in Maryland, California, the Sandwich Islands and Japan established by international coöperation to prosecute this work systematically.

THE NEW COSMOGONY.*

J. E. GORE.

In Laplace's Nebular Hypothesis, the original mass, from which the solar system was evolved, was supposed to consist of gaseous or meteoroidal material filling a space of spheroidal form

* KNOWLEDGE AND SCIENTIFIC NEWS. September, 1906.

and extending to the orbit of the planet Neptune, or somewhat beyond it. If a gaseous state be assumed the whole mass was supposed to be in hydrodynamical equilibrium, and rotating in a period equal to the period of revolution of the present farthest planet. We might also assume that the original mass consisted of a gigantic swarm of meteorites, for Professor G. H. Darwin has shown that such a swarm would have nearly the properties of a gas. On either assumption the mass would contract by its own gravitation, and, the angular velocity gradually increasing, the centrifugal force would in time flatten the spheroidal mass at the poles. From this flattened spheroid Laplace thought that rings would be detached at certain intervals, and these rings consolidating would eventually form the planets and satellites of the solar system as we now see them.

It has been shown, however, by Mr. F. R. Moulton, that the matter detached from the rotating gaseous spheroid would be "shed continually," and that no separate rings could be formed. This would occur whether we consider the original mass to have been gaseous or composed of meteorites. But supposing the rings to have been, by some miracle, detached from the parent mass, we should expect to find that the plane of Mercury's orbit would deviate less than the other planets from the average plane of the solar system; also that the orbits of the "terrestrial planets," Mercury, Venus, the Earth, and Mars, would be less eccentric, that is, more nearly circular, than those of the outer planets. But the known facts concerning Mercury's orbit are quite opposed to these conclusions. The inclination of its orbit to the plane of the ecliptic (7°) is greater than any of the large planets, and the eccentricity of its orbit (0.20) is only exceeded by that of some of the minor planets between Mars and Jupiter. Further, Moulton shows that the distribution of masses among the planets of the solar system indicates that the original nebulous mass must have been very heterogeneous and not homogeneous as Laplace's theory postulates.*

There are numerous other difficulties connected with Laplace's Hypothesis, and many attempts have been made to overcome them. But these efforts have proved only partially successful, and for some years past, it has become increasingly evident that the hypothesis must be abandoned for something in better agreement with modern telescopic discoveries. The idea that the planets were formed by the condensation of rings detached from a nebulous mass is an hypothesis for which we find no warrant

* *Astrophysical Journal*, March, 1900.

in the heavens. Laplace's idea of a Nebular Hypothesis was probably suggested by a consideration of Saturn's rings. But modern researches on tidal action tend to show that this wonderful system was not originally formed as a ring left behind by Saturn during the progress of condensation from the nebulous stage. More probably the matter composing the rings was originally separated from the planet in one mass. This mass being too close to Saturn to consolidate into a satellite—being within what is known as "Roche's limit"—was torn into fragments by the force of tidal action, and its particles were scattered round the planet in the form of a ring as we now see it. On this view of the matter the course of events was exactly the reverse of what was supposed to have happened in Laplace's Hypothesis. Instead of a ring being first formed, and then a number of small satellites from this ring, we must now conclude that a mass of matter was first detached from the partially consolidated planet, and that then this mass was broken up into small fragments by the enormous tidal action of the central mass.

We see in the heavens numerous forms of nebulæ—spiral nebulæ, planetary nebulæ, etc.—but there is no real example of a ring nebula. Those which have been termed "annular nebulæ" are most probably spiral nebulæ seen foreshortened. Of the numerous nebulæ recently discovered with the Crossley reflector at the Lick Observatory it has been found that "a large proportion are spiral, and that practically, all the spirals are lenticular or disk-shaped. Many of them are relatively very thin."* At one time the photographs of the great nebula in Andromeda were thought to show signs of ring formation, but Dr. Roberts, describing his photograph of this wonderful nebula, says: "That this nebula is a left-handed spiral and not annular, as I at first suspected, cannot now be questioned; for the convolutions can be traced up to to the nucleus, which resembles a small bright star at the center of the dense surrounding nebulosity." Even the "ring nebula" in Lyra, which is sometimes adduced as an example of ring formation, was found by Professor Schaeberle, of the Lick Observatory, to be "a two-branched spiral which commences at the central star, and in a clockwise direction emerges on opposite sides near the minor axis."† Even the apparent ring form of this nebula seems to be fictitious. Instead of being annular in shape, it appears to be a hollow spheroid, the ring representing the thickness of the shell. To anyone who still persists in maintain-

* Publications of the Astronomical Society of the Pacific, Dec. 10, 1904.
† *Nature*, August 6, 1903.

ing the theory of ring formation in nebulæ it may be said that the whole heavens are against him.

It has always been difficult to imagine how rings could possibly be formed from the parent mass, considering the extreme tenuity of the original nebula. Computing from the total mass of the bodies composing the solar system, the density of the primitive nebulous mass—supposing it extended to the orbit of Neptune—would have been almost inconceivably small. The density of atmospheric air would be millions of times greater, and how rings could be formed in such a tenuous gas has always been a serious difficulty in the discussion of Laplace's Hypothesis. But a still more fatal objection has lately been found. The original idea was that the detached "rings" would at first break up into separate masses, and that these fragments would afterwards —by their mutual attraction—consolidate into planets. But from a mathematical investigation recently undertaken by the well-known American mathematician, Mr. John N. Stockwell, he finds that if two masses "are moving in the same plane and at the same mean distance from the Sun, and are situated at an angular distance greater than 60° and less than 180° from each other, as viewed from the Sun, their mutual perturbations will cause them to approach each other until the distance apart becomes equal to 60°;" and, further, if the two masses "are situated at an angular distance of less than 60° apart, as seen from the Sun, their mutual perturbations will cause them to recede from each other until their distance apart becomes equal to 60°; and they will always remain in a condition of stable equilibrium at that distance apart, and will revolve round the Sun forever free from mutual disturbance."*

This result seems fatal to Laplace's Hypothesis in its original form, as the fragments of the ruptured ring could never have consolidated into a single planet. Mr. Stockwell adds: "The assumption by Laplace that the matter of which the ring was composed would concentrate by the mutual attraction of its different parts into a single planet or satellite is, therefore, not sustained by a rigorous calculation; and since the Nebular Hypothesis wholly fails to satisfy that requirement it evidently rests on no logical foundation."

Compelled, therefore, as we apparently are, to abandon

Astrophysical Journal No. 557, March 4, 1904. An asteroid recently discovered (TG) has nearly the same period and mean distance as Jupiter, and seems to fulfill the conditions supposed by Stockwell. At present it is 60° from Jupiter, and may perhaps remain so.

Laplace's Nebular Hypothesis in its original form, are we, therefore, obliged to relinquish all attempts to explain the formation of suns and solar systems from the consolidation of gaseous matter? By no means. The heavens, which are clearly against Laplace's Hypothesis, are strongly in favor of a new theory, a new cosmogony, which will probably stand the test of mathematical analysis. This is the evolution of suns and systems from spiral nebulæ. Of the half-million nebulæ discovered with the Crossley reflector a large proportion are spiral, and the study of these remarkable and interesting objects will probably form an important portion of the work of future astronomers.

Laplace's original nebula was gaseous, and a gaseous spectrum shows bright lines. But the spectrum of the spiral nebulæ is *continuous*, indicating that they have partially consolidated from the gaseous state. We can, therefore, easily imagine that masses might be thrown off or detached from the parent mass by the centrifugal force of the rotation. This seems much more probable than the formation of rings from a highly tenuous nebula. Photographs of spiral nebulæ show us masses in the act of being detached from the spiral branches. This is particularly noticeable in the photograph of the great spiral in Canes Venatici (51 Messier), in which we see the process going on before our eyes.

The theory of the evolution of suns and solar systems from spiral nebulæ has recently been investigated mathematically by Professor T. C. Chamberlin and Mr. F. R. Moulton. This investigation consists in an attempt "to test by an appeal to the laws of dynamics the consistency of the ring theory with known phenomena. Contradictions were uniformly found, and in some cases the results were so conclusive as to compel us frankly to abandon it as an untenable hypothesis."* An outline of this new cosmogony, called by Professor Chamberlin the "Planetesimal Hypothesis," and which certainly seems a great improvement on that of Laplace, may prove of interest to the general reader.

The *origin* of the nebulous mass from which the solar system is supposed to have been evolved was not considered by Laplace. He *assumed* its existence, and then proceeded to show, as he thought, how the Sun and planets might have been formed from it by the consolidation of the nebulous matter in the course of ages. The origin of spiral nebulæ is, of course, unknown, but of their existence there can be no doubt. Photography has fully confirmed the discovery originally made by Lord Rosse with the giant six-feet telescope. They are very numerous in the heavens.

* *Astrophysical Journal*, October, 1905.

Professor Keeler thought that probably one-half of the nebulæ found with the Crossley reflector are spiral, and that "any small compact nebula not showing evidence of spiral structure appears exceptional." Spiral nebulæ were of course, unknown to Laplace, and had he known of their existence we should probably never have heard of "ring formation."

A spiral nebula may possibly have been formed by a "grazing collision" of two solid or nebulous masses, or by the near approach of two bright stars. Supposing the near approach of a large body to another of larger size, the effect on the latter body would be the production of a gigantic tide on the side turned towards the approaching body, and another tide of almost equal size on *opposite* side. These tides would produce explosions and eruptions of nebulous matter from the interior of the star, and Moulton shows that the ejected material would assume the spiral form. That a spiral form would be assumed by a rotating gaseous mass has also been shown by Herr E. J. Wilczynski, of Berlin.* Now it is a remarkable feature of spiral nebulæ that the spiral branches (usually two) almost invariably issue from the central nucleus at diametrically opposite points, thus agreeing with the new hypothesis. The spiral nebulæ which we see in the heavens are, of course, constructed on a colossal scale, and probably represent a stage in the evolution of star systems rather than solar systems like ours. But the principle would be the same in both cases.

In Chamberlin's "Planetesimal Hypothesis" the original nebulous mass "must have spread out in the form of a relatively thin disk," and instead of being homogeneous, as in Laplace's Hypothesis, "it may have had almost any degree of heterogeneity." In this theory fluid pressure, which was of fundamental importance in Laplace's Hypothesis, is of minor consideration, "and no general shrinkage with loss of heat" plays any part in the evolution of planets from the parent mass.

Moulton shows that on this theory the resulting planets will all probably revolve round the nucleus in the same direction as the original rotation, and that the planes of their orbits "will nearly, though not exactly, coincide;" almost that the orbits of the larger planets will show smaller deviations from the general plane than those of the smaller planets, like Mercury and the asteroids. This we know to be the case in the solar system. He shows that the present rotation of the Sun is due to the original rotation of the mass from which it was formed, combined with

* *Astrophysical Journal*, Vol. 4 (1896), p. 98.

the disturbance caused by the body which approached it, and that the more rapid rotation of the Sun's equator is due to the same cause. He also shows that the larger the planet "the more nearly circular in general" its orbit would be; and this also agrees with the known facts of the solar system. The orbits of the so-called "terrestrial planets," Mercury, Venus, the Earth, and Mars, are, on the average, more eccentric than those of the large planets, Jupiter, Saturn, Uranus, and Neptune; and those of the small minor planets between Mars and Jupiter are still more so.

With reference to the rotations of the planets on their axes, Moulton shows that these would probably be direct, that is, in the same direction as the orbital revolutions, and that the inclinations of the axes of rotation to the planes of the orbits might be different for different planets. Any well-marked deviation from this rule might be expected in the outer planets of the system. This we see in the case of Uranus and Neptune, which have always proved stumbling blocks in Laplace's Hypothesis. We should also expect, he thinks, "to find the larger planets rotating more rapidly than the smaller," and this we know to be the case.

With reference to the satellites, the direction of their motion round the primary might, on the new hypothesis, be either direct or retrograde, according to the circumstances. Retrograde motion might be expected in satellites very remote from their primary and revolving in orbits of high eccentricity. This is the case with Phoebe, the recently-discovered ninth satellite of Saturn. The position of the satellites of Uranus could not have been predicted by the new theory, but Moulton thinks that "they do not definitely contradict it as they do the ring theory."

A satellite might also, on the new theory, revolve more rapidly round its primary than the primary rotates on its axis. This is the case with Phobos, the inner satellite of Mars, which revolves round the planet in 7 hours, 39 minutes, 15 seconds, while Mars' period of rotation on its axis is 24 hours, 37 minutes, 22.66 seconds, or over three times as long. This usually rapid rotation of a satellite formed another objection to Laplace's Hypothesis, but it seems to be consistent with the new cosmogony.

With reference to the so-called "moment of momentum" of the solar system, Professor Chamberlin has shown that the greater portion belongs to the planets, and that Jupiter alone contains about 95 per cent of the moment of momentum of the total mass within the orbit of Saturn. This is, according to Moulton "an inevitable consequence of the spiral theory but, on the contrary,

the whole question of the moment of momentum is a rock on which the ring theory breaks."

According to the new cosmogony the outer portions of the matter ejected from the original body would evidently be formed from the surface portions of the star, while the matter which followed would "come mainly from lower depths," and would probably consist of materials of greater density. The smaller planets should, therefore, be cool and of high density, and the larger planets hot and of small density. This is also in agreement with the known facts of the solar system. The average density of Mercury, Venus, the Earth, and Mars is about 4½ (water=1), while the mean density of Jupiter, Saturn, Uranus, and Neptune is only 1.03, or about that of water. We know that the Earth is cool, and that probably Mercury, Venus, and Mars are so also, while there is good reason to suppose that the large planets are in a highly heated condition.

On the whole, Moulton concludes that "the spiral theory is even now a good working hypothesis." It seems to explain satisfactorily all the observed phenomena upon which the ring theory was based, and many others which are in contradiction to Laplace's original hypothesis. "Nothing has yet been found which seems seriously to question its validity."

The new cosmogony will, of course, raise many very difficult questions in celestial mechanics, and will give a considerable amount of work to mathematical astronomers before it can be placed upon a satisfactory basis; but the work which has been already done by Chamberlin and Moulton shows clearly that the spiral theory is far superior to Laplace's Nebular Hypothesis, which should now be definitely abandoned and consigned to the limbo of unproved theories. The heavens show us thousands of spiral nebulæ, which are evidently in a state of rotation round a central nucleus, but which will probably take ages before they have finally consolidated into suns and solar systems. But ages are but moments in the evolution of the stars, and we need not expect to find much evidence of rotation and consolidation during the brief span of human history. Empires rise and fall, dynasties are founded and dissolved, but the heavens move on in their silent course, and the human race will probably have perished before the universe has reached its final density.

THE STRUVE DOUBLE STARS BY LEWIS.

S. W. BURNHAM.

For Popular Astronomy.

There was a time, and not so very long ago, when the work of the great Greenwich Observatory was confined to meridian observations of the stars and the principal members of the solar system. For a century or more micrometrical observations of the stellar systems form an insignificant part of the huge volumes of observations turned out each year by the leading observatory of the world. Doubtless the fundamental work of position was considered of the first importance; and doubtless for many years this was the correct view, but the time came long ago when it seemed strange that the micrometer was not more in evidence, even with the equatorial instruments then in use. But later on the observatory acquired a much better and larger telescope for one thing, and, what is perhaps of more consequence in this connection, a new head for the institution, supplemented by a staff of capable and zealous workers, who could see work worth doing with other than meridian instruments. And so for the past ten years or more the 28-inch equatorial has turned out a large quantity of valuable work on double stars; and evidently these observations with the micrometer are regarded as a prominent part of the regular work of the observatory. The measures to this time have been made principally by Lewis, Bowyer and and Bryant from a well selected working-list of double stars, largely from the older catalogues. Very little work of this class is now regularly done in Great Britain, and still less at the observatories on the continent.

Mr. Lewis has found time to collect and arrange the principal observations of the Struve stars from the earliest date down to the present, thus giving a history of each pair through the successive measures, commencing with Sir William Herschel, and ending with the latest results found at the Greenwich Observatory. This makes a book of some seven hundred pages, and is issued as Vol. LVI of the *Memoirs of the Royal Astronomical Society*. This will be a useful reference work to all who are interested in the subject matter, since it presents at sight, by the chronological arrangement of the measures, the evidence of motion, or the absence of it, in the components of each of the 3134 pairs. The various orbits which have been computed for the

binary systems are referred to, with such comments on their value as would be warranted by the observations of recent date. The principal star is identified in some of the various star catalogues, and when the meridian positions furnish reliable proper motions these values are given, and compared with those shown by the direct measures, where the relative change is evidently due to the proper motion of one or both of the components.

If it was worth while to criticize in minor detail a work so generally valuable and complete, attention might be called to the fact that while all the observations are not given, the omission of a large number of the measures of the much measured pairs, and particularly the omission of all the one-night positions, would not only have reduced the size of the volume, but made it more useful for consultation. It goes without saying that many thousands of the measures of the Struve stars are neither useful nor needed, for various reasons which the author of this volume will readily appreciate. It would have been much better to have omitted the superfluous and second-class material, and given a reference to the place of publication of all the measures of each star, without reference to whether or not they are cited in the tabular list of observations.

In meridian work it is of course all right to give proper motions in the two components of right ascension and declination, but for use in connection with relations given in distance and angle, it would have been much more practically useful if this motion in space had been expressed in arc and position-angle, since this has to be done by the reader in order to see what it really means. To be sure it is but a little more than half a minute's work with the slide-rule to find the result to the nearest thousandth of a second of arc, and the nearest tenth of a degree in direction if one is willing to stop at each star and make the conversion.

A reference to the prominent naked-eye stars contained in this volume would have been easier if the usual designation had formed part of the title along with the Struve number, instead of being mentioned in the reading matter which follows. The bright double stars of the several constellations will always be referred to by their usual names, and not by the number in *Mensurae Micrometricae*, and consequently in this, and many other works on the same subject, one has to look for *Castor*, γ *Virginis*, 70 *Ophiuchi*, etc., unless he should happen to remember that these are respectively Nos. 1110, 1672 and 2272 of Struve's great catalogue.

In his introduction Mr. Lewis has given in a condensed and

useful shape the more interesting facts from *Mensurae Micrometricae* concerning the number of pairs in the several classes and colors, the colors of the components, number of stars rejected for various reasons, and the general plan, arrangement and scope of the work. This will be of interest to those who do not possess a copy of this somewhat rare catalogue. The volume is further enriched by a fine portrait of the great double star observer as he appeared in 1844, reproduced from a picture in the possession of the Royal Observatory.

Yerkes Observatory,
Williams Bay, Wis.

HOW THE POSITION OF NEPTUNE WAS ORIGINALLY COMPUTED.

WILLIAM H. PICKERING.

FOR POPULAR ASTRONOMY.

The discovery of Neptune is justly regarded as one of the greatest triumphs of Astronomy. How the exact position in the heavens of a great planet that had never been seen could be predicted from the slight displacement of another body itself barely visible to the naked eye, seems to the average mind almost incomprehensible. It is generally supposed that the computations involved are so abstruse and complicated as to be quite beyond the reach of the average intellect, and the writers of the text-books do not even attempt to explain them.

That such is far from being the case it is the object of the present writer to point out. Indeed so far as the general principle of the computation is concerned we shall find that the mathematical work involved leading to the discovery might have been made with a little instruction, and a few facts, by almost any high school graduate. It is possible to apply the higher mathematics even to the affairs of every day life, but many people get along very well without it. So with astronomy, many of its most complicated problems may be approximately solved in the simplest manner if we only use a little thought and common sense.

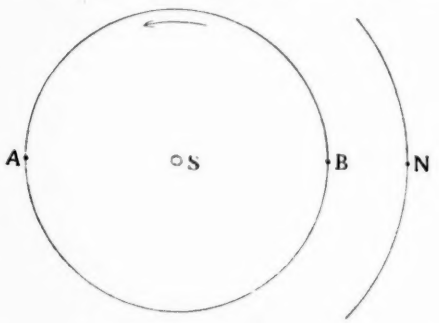
But besides predicting the ephemeris, that is, the place of Neptune in the sky, Adams and Le Verrier also attempted to compute its mass, and the elements of its orbit. This requires a much higher grade of mathematics than to determine its place, and in this effort they both failed. This was due to the fact that they

evidently were not aware that their data were insufficient for the purpose.

In order to predict the position of Neptune in three coördinates, two assumptions and a computation were involved. The first assumption was that the plane of its orbit coincided with that of Uranus. This assumption proved to be sufficiently correct, and settled the latitude of the assumed planet.

The second assumption was that its mean distance from the Sun was double that of Uranus. This would give a result complying approximately with Bode's law. This assumption proved to be erroneous, and though the value was later slightly reduced by both computers, the error was still so great that it entirely vitiated the computation of the remaining elements of the orbit. It fortunately did not appreciably influence the computed ephemeris however, because it mainly affected the distance of the assumed body.

The computation may best be understood by an inspection of the accompanying figure. Let S be the position of the Sun, and N that of the unknown planet. In order to determine its longitude let us consider the circle AB to be the orbit of another planet. While moving from A to B this planet will constantly be pulled



forward by N. While moving from B to A it will constantly be pulled backward. As a result it will be farthest ahead of its place when its heliocentric longitude is the same as that of N and farthest behind when in the opposite part of its orbit. It may be remarked that the displacement at A is insignificant as compared with that at B.

It was found that Uranus kept getting ahead of its computed position until 1822. After that date it kept falling more and more behind it. The heliocentric longitude of the unknown body

in 1822 was therefore the same as that of Uranus in the same year. Given that fact, and the mean distance of Neptune being assumed, it was easy to compute by Kepler's third law the longitude of Neptune in 1846. The error in this assumption was not sufficiently great to cause a large error in the few years that had elapsed since 1822, so it turned out that the computed ephemeris of Neptune was nearly correct, although the elements of its orbit were entirely erroneous.

The late Professor Benjamin Peirce claimed that since the elements of the computed orbit were erroneous and the real planet was therefore some 280,000,000 miles away from its computed place that the real planet was not the planet of Adams and LeVerrier at all, and that the discovery was little more than a happy accident. This view did not commend itself to Adams and LeVerrier, and caused some little feeling between them and its author. It was really unfair, for as we have seen, their ephemeris was nearly correct, and was obtained in a perfectly legitimate manner. Had Neptune happened to be some years further advanced in its orbit, so that its conjunction with Uranus had occurred, let us say, in 1890 instead of in 1822, Adams and LeVerrier would never have discovered it, but some of the astronomers of the present day would have computed a correct ephemeris and have made the discovery, and would also probably have made the same erroneous assumption as to its distance and the elements of its orbit.

Harvard Observatory,
September 8, 1906.

YERKES OBSERVATOTY. II.

W. W. PAYNE.

The work going on at Yerkes Observatory as we saw it in August last impressed us so strongly and so fully that we did not say last month nearly all about it that it deserves or that was in our mind to say.

We were speaking of Mr. J. A. Parkhurst's work when we stopped in the last issue. We did not feel sure that Mr. Parkhurst would be able to finish some writing about his work that he was doing for us in time for that number, but much to our gratification he did get it ready. In view of that article which was made the leader in our October issue, our readers have already obtained a good idea of the kind of work he is doing and

something of the importance of it. Those who have read the article carefully will have noticed the great help that photography affords for the study of faint variable stars in fields of nebulosity. This field of work is practically new, because there has been heretofore no means of separating the nebulosity surrounding the faint stars from the stars themselves, so that the latter might stand out clearly and individually alone for study and precise measurement. If one examines carefully the frontispiece of our last number, it will be seen what is meant by separating faint stars from the nebulosity in which they are involved. It must be acknowledged that the results are surprisingly good and those who have undertaken this new line of work must be greatly encouraged in the hope of what is certainly ahead of them.

The interesting point in all this, for readers who have not had knowledge of this work already, is, how has Mr. Parkhurst been able to separate these faint stars from the nebulous masses surrounding them? The great difficulty is measuring such stars accurately for variation while embedded in nebulous light in the bright background of the nebula which flickers with the least unsteadiness in the atmosphere making the estimates for magnitude very uncertain. It was then found very necessary to overcome these difficulties by screening the light of the nebulous matter from that of the stars. Here again comes the needed help, in a very difficult problem, from Mr. R. James Wallace, the expert photographer of the Observatory. It was known to these observers that nebular light was mostly concentrated in the bright lines of the spectrum whose wave-lengths are 5007 and 4959. These are the first and second nebular lines of the hydrogen series, and the ultra-violet line in wave-length 3727. Now from Mr. Wallace's practical knowledge of the photographic action of dyes, he was able to make a filter which would transmit only a band of the blue light between the second and third hydrogen lines, their limits being about 4860 and 4640. This was a dry filter and it was placed nearly in contact with the sensitive plate from which arrangement the result was secured as shown in figure 2 of the frontispiece of our last number. We have called attention again to this important matter to emphasize the possibilities now before the skilful worker with light-filters. It seems to us that Mr. Parkhurst is right in saying that the possibilities in this new field are almost boundless. The working point is skill in photography.

One other thing has occurred to us in this connection which we barely mention now, for we do not know enough about it to say

much. Others may know more, and may have done some work in the same direction, which would give valuable information if the suggestion has in it any possibility of merit. It is this: Is it possible to make screens which will destroy harmful air waves, or neutralize them so that observers may be able to control or regulate the 'seeing' in times of great unsteadiness of the atmosphere? Of course we know the answer that is often made to this question; viz: that it is impossible for a person at the bottom of a sea of air fifty miles deep to expect to control waves in or near its surface by trying to calm those waves within his reach, which at most can be only a few feet from the bottom. The question still recurs, is it impossible? This reference is a digression, and not at all related to the astronomical work now under consideration.

The other thought in Mr. Parkhurst's paper is a very fruitful one. It is his view of the importance of photography in determining star places of precision. It has come to this, now, that catalogues must have better star places if astronomy is to go forward with the work immediately before it that ought to be done speedily and well for there is in sight so much to do.

The photographic work done on the planet Eros, with all the new questions it raised not yet fully solved, has given to astronomers much confidence in it as a means of record involving, at least, a very good degree of exactness. The reasons for this view are, the photographic film does not change on the glass, the accuracy of the measuring machines and the leisure and opportunity the observer has in measuring and determining his results. It seems probable that Mr. Parkhurst's view that the photographic results generally will equal, if not excel those of the micrometer or the heliometer is correct.

Other work at the Yerkes Observatory that we had opportunity to look into was that of S. W. Burnham. His field is, of course, that of double stars. He is now observing with the great forty-inch refractor all the doubles that need present attention to make his forth-coming volume of double stars as complete as possible. With all the help he has from other leading observatories in the United States, and with all the time he can get for observing himself, the field is so large, and there is so much that is new to be added to the old that Mr. Burnham's task of making the completest record of double stars possible is a large one. That his work now on double stars is by far the most complete and most reliable we think is admitted by all who know the present conditions of double star observing.

THE WHARTON REFLEX ZENITH TUBE.

ERIC DOOLITTLE.

For Popular Astronomy.

When the Flower Observatory was built, ten years ago, the first instrument set up ready for use was a very fine zenith telescope. With this instrument, the largest of its kind so far made, Professor C. L. Doolittle at once began observations for variation of latitude, and the series has been kept up without interruption until the present time.

These observations were a continuation of those carried on in South Bethlehem from 1888 to 1895 and were made in the same manner. Four lists of stars were compiled, each list containing from eight to twelve pairs, so selected that the difference between the mean right ascensions of the stars on any two successive lists should be about six hours. Thus on each night of the year two such lists may be observed, the first in the early part of the night and the second six hours later. Since all stars come to the meridian about four minutes earlier on each successive night, the first list will in time be lost in the evening twilight. The second list is then taken for the evening series and the third list for the morning one, the latter in time being replaced by the fourth list, and this afterward by the first and the observations so continued indefinitely. This arrangement, first proposed by Dr. Küstner in 1890, enables the constant of aberration to be determined independently of the variation of latitude.

There are two striking results which are invariably found in long series of zenith telescope observations of this kind. In the first place, the value found for the constant of aberration is always considerably larger than that obtained by a judicious combination of the values from all other sources. For many years the result 20″.4451 found by Struve was employed, although it has been certain for some time that this is considerably too small. At the Paris conference of May 1896, the value 20″.4700 was selected as being the best now obtainable. In the American Ephemeris the star numbers and the apparent places of the fixed stars are now published computed with both the old and the new system of constants. During the past twenty years Professor Doolittle has never obtained from his observations a value of the constant of aberration so small as either of these;

the most probable result from all of his work up to the present is 20″.54.

The second anomaly which is found in practically all extended series of observations for latitude is an occasional apparent daily change. Each pair of stars observed furnishes a determination of the latitude and it is sometimes found that every value so determined on one night is greater or less than every value on preceding and following nights. For example, the following individual values were obtained for the latitude on three successive nights in 1902:

	Feb. 4	Feb. 5	Feb. 6
	″	″	″
39° 58′ +	2.23	2.00	2.23
	2.15	1.66	2.21
	2.25	2.04	2.40
	2.16	1.92	1.95
	2.32	1.85	2.09
	2.60	1.64	1.94
	2.17	1.80	2.21
	1.88	1.64	2.00
	2.06	1.91	2.01
	2.20	1.70	2.12
Mean	2.20	1.82	2.12

It will be seen that the latitudes of Feb. 5 are throughout smaller than those of Feb. 4 and Feb. 6, the average difference being 0″.34. If this can be regarded as real, it indicates that on Feb. 5 the instrument was 29 feet further south, that is 29 feet further from the pole of rotation of the Earth, than on the other two dates.

Such differences are considerably too large to be ascribed to accidental errors of observation merely, but from exactly what cause they arise is not yet known. In some of the many papers published on this subject it is suggested that they may be due to changes of refraction caused by an unusually high or low barometric pressure either north or south of the observatory. Such an atmospheric condition might equally tilt up all the strata of air in the vicinity and thus alter the refraction in such a way that all the rays of light from the stars observed would be bent toward the south or north. This would of course produce a change in each value determined for the latitude and the changes would all be in the same direction.

Again, it has been suggested that the source must be looked for in the instrument itself, though what the nature of the constant instrumental error may be is not evident. Of course in all zenith telescope work everything depends on the performance of the levels; an error in level reading will affect the resulting latitude

by its whole amount, as evidently must be the case since it is from the level alone that the horizon plane is determined. This is also the case where latitude is determined by the transit instrument in the prime vertical, as in the long and valuable series at present being taken by Dr. G. H. Hill. The same kind of anomalous results must be expected in any instrument employing a level if it is here that the difficulty lies.

It does not appear probable, however, that the difficulty can be looked for in the levels. Were one end of the level tube warmer than the other, this would produce a constant level error but since the instrument is reversed at least once with each pair of stars this error will be wholly eliminated. If one half of the observing room, for example the north half, were warmer than the south, this would theoretically produce a constant error because the end of the level tube toward the north would always be warmer than the other. But when we consider the shortness of the level tube and the short interval of time between reversals this explanation seems wholly inadequate, especially as the entire room is so open that the instrument is practically in the open air. Professor Doolittle has two accurate thermometers, one mounted about five feet north of the pier and the other the same distance south. These seldom show an appreciable difference. For example, on the dates of the observations given above the readings were as follows:

	North.	South.
	°	°
Feb. 4	+ 14.5	+ 14.7
Feb. 5	+ 10.9	+ 11.3
Feb. 6	+ 20.0	+ 20.2

The differences in the temperatures of the two ends of the level tube would probably have been very much less than this, even if the instrument had not been reversed during the whole time of observation.

Finally, there remains the supposition that the observed differences are real and that the latitude of an observatory may actually shift twenty or thirty feet in the course of a day or two. This apparent shift may be caused either by a displacement of the pole of rotation of the Earth or it may be local. It is conceivable that the former hypothesis is the true one and that the change in the position of the pole may be due to the movement of storms on the Earth; yet because so great a displacement would probably become evident in unusual tides, and because thus far there is no evidence of a large daily displacement from observations made simultaneously at different observatories,

Professor Doolittle regards this hypothesis as improbable. He is inclined to believe that at least a part of the change may be due to a local distortion of the crust of the Earth.

To thoroughly investigate the anomalous changes in latitude, two additional instruments are desirable. First, some instrument of a wholly different type from the zenith telescope which shall be mounted near the latter so that observations may be taken simultaneously with both instruments. If like changes should be found with each of them, it would of course be evident that whatever the cause of these changes might be it could not lie in the zenith telescope itself. And second, when this has been ascertained, it seems very desirable that a third instrument

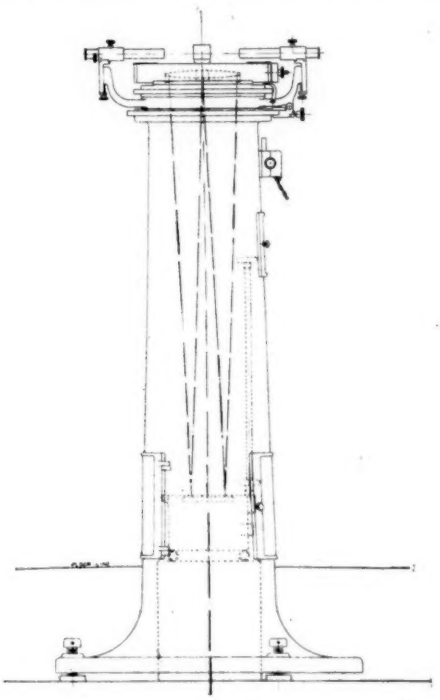


Figure 1

should be mounted approximately in the same meridian and about forty or fifty miles away from the others in order to discover over how large an area the change extends.

Toward the end of the year 1903, through the liberality of

Mr. Joseph Wharton of Philadelphia, funds were provided for erecting a second latitude instrument at the Flower Observatory. This instrument was designed by Professor Doolittle according to the principles outlined by Airy in 1848. The Greenwich instrument was constructed in 1851 for the purpose of observing the star γ Draconis: it was at that time named by Airy the Reflex Zenith Tube. These two are the only two zenith tubes which have ever been built.

A sectional view of the Wharton Zenith Tube is shown in Figure 1. The heavy telescope tube rests on three leveling screws

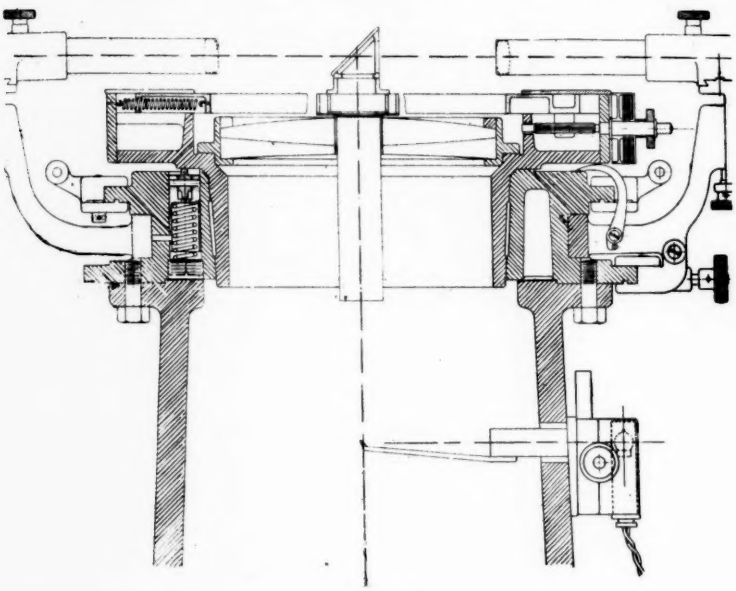


Figure 2

by means of which it is so adjusted that its optical axis is exactly vertical. On the upper end of the tube rests the 8-inch objective; inside of the tube at a distance below the objective equal to slightly less than half the focal length there is a hemispherical bowl of mercury which floats in a larger basin of mercury and this, surrounded by woolen packing, rests in a box of sand on the pier. Through the exact center of the objective a hole 1.5 inches in diameter is bored; the difficult and dangerous work of drilling this hole in a figured lens was performed by Mr. Brashear who made all of the optical parts of the instrument, the moun-

ting being constructed by Warner and Swasey.

The whole instrument resembles a telescope which is cut in two half way between the eyepiece and the objective, the large end of which is then mounted to point exactly to the zenith. The rays of light from a zenith star first pass through the objective and are then reflected up from the mercury surface through the hole in the objective, being finally brought to a focus about 0.25 inches above the center of the lens.

The upper end of the instrument is shown in Fiure 2. The micrometer frame is above the objective itself the wires lying 0.25 inches above the objective in the focal plane. The arrangement for illuminating the wires is shown below and to the right. The micrometer box together with the objective and its cell can be rotated about the optical axis. When these are in the position shown in Figure 2, the image of the star and the wires after reflection from the 45° prism will be seen in the telescope on the right. Upon rotating the entire apparatus which rests on the tube through 180°, (the telescopes themselves being fixed), the image is received in the telescope on the left.

As the instrument is of so novel a form, many interesting and puzzling points were met with during its construction. Not the least of these were connected with the mercury surface. After correspondence with astronomers who had had experience with such surfaces, Professor Doolittle was at one time seriously apprehensive lest it might be wholly impossible to secure a surface sufficiently free from tremors under so large an objective. But the device above described, together with that of beveling the edge of the hemispherical bowl so that the slight waves in the mercury will be destroyed upon reaching the edge and not reflected back over the surface, was found to wholly remove all difficulty. The images are perfectly clear and steady, very little if at all inferior to those obtained in a telescope which is mounted in the usual way.

Another interesting device is that by which the instrument is focussed. It would evidently be impossible to bring the wires into coincidence with the focal plane by moving them out and in from the objective. Instead of this the principal focus itself is moved up or down by forcing in or drawing off mercury from the reservoir surrounding the hemispherical bowl. If the wires are above the focal plane, a slight amount of mercury is added; otherwise a small amount is removed.

Although the hole through the objective, the micrometer frame, and the prism above it cut off considerable light, it is found that

there is no difficulty in observing stars of the 8.5 magnitude. Since the whole field of the instrument is but 20′ it is impossible to observe any star with it whose zenith distance exceeds 10′. It was therefore necessary to make the lens large enough to show the fainter stars so that a sufficient number could be found for observation. It has not so far been necessary to add any star to the observing list whose zenith distance exceeds 300″.

The method of observing is as follows. With the instrument set as in Fig. 2, the observer looking through the telescope on the right, would see a close zenith star enter the field, its direction of motion being parallel to the movable micrometer wire. There are two groups of fixed threads in the field placed at right angles to the micrometer thread. When the star reaches each fixed thread of the first group, successively, it is bisected with the micrometer thread and the reading taken. The instrument is then reversed, and the observer, (now looking through the telescope on the left), sees the star approaching the same group of threads as before, which by the reversal has been brought ahead of the star to the other side of the field. As it crosses these threads a second time, readings are taken as before; this completes the observation. When the star is next observed on some succeeding night, the telescope on the left will be used first and that on the right last; the second group of fixed threads will thus be employed. The interval between the last bisection in one position and the first after reversal is 50 seconds; this allows just time enough to make the record and to reverse.

Reversing the instrument does not eliminate the errors of the micrometer nor the error which arises because the plane of the objective is not truly horizontal. But this latter error is very minute. A level is rigidly connected with the cell of the objective for testing its adjustment in this respect and it is found that a change of twelve divisions in the reading of this level only produces a change of 0″.01 in the latitude. In the zenith tube therefore the level is practically eliminated; the entire determination of latitude depends only on the performance of the micrometer and the accuracy of the bisections with the micrometer wire.

Professor Doolittle has mounted this instrument about 12 feet due east of the zenith telescope and under the same roof with it. His lists of stars are so arranged that he can employ both instruments at once, passing back and forth from one to the other as the stars to be observed in each come to the meridian. The two sets of observations are therefore practically simultaneous. If daily changes in the latitude exist, these will be shown by both

instruments. And in addition it will be a matter of great interest to see how the value for the constant of aberration as determined with the reflex zenith tube compares with those which have hitherto been found.

The positions of many of the stars on the new lists cannot be accurately obtained from the catalogues; Professor Tucker of the Lick Observatory is at present observing these with the meridian circle. Therefore, except for the general statement that its performance seems to be satisfactory, Professor Doolittle cannot yet tell what the observations with the new instrument will show. By the middle of next December the first two years' series of observations will be completed and it is hoped that soon after this a more definite statement can be made.

Upper Darby, Pa.

ON THE OBSERVATION OF VARIABLE STARS.

II. The Reduction of Observations.

P. S. YENDELL.

For Popular Astronomy.

In the former paper, I described the processes of observation by the Argelander method, the precautions to be observed, and the principal sources of error to be guarded against in conducting them. I shall now go on to show by a working example the manner in which these observations may be put into numerical form, so that their indications of the variability of the stars, and of the course of their variations may be made apparent.

For this purpose, I have extracted from my notes, with some omissions and modifications, made for the purpose of simplifying the example, a series of observations of o (*Mira*) Ceti, made near the maximum which occurred early in 1896.

The comparison-stars used are

			M
a =	F 75	Ceti,	5.48
b =	ν	"	5.06
c =	ξ^1	"	4.70
d =	ξ^2	"	4.44
e =	δ	"	4.04
f =	*a*	"	3.85
g =	γ	"	3.89

For convenience's sake, the stars are in this paper designated by the letters assigned them in the first column of the above table. In the last column the photometric magnitudes of the several stars, from the Potsdam Photometric measures, are given.

The list of observation is as follows:

1895 Dec.	1	*b* 2 v,	v 4 *a*	
1896 Jan.	4	*d* 4 v,	v *c*,	v 4 *b*
	5	*e* 5 v,	*d* 3 v,	v 1 *c*
	6	*e* 3 v,	*d* 1 v,	v 2 *c*

1896 Jan.	11	*d* 1 v,	v 2 *c*,		
	16	v *f* ,	v 1 *e*,	*g* 2 v	(naked eye)
	22	*g* 1 v,	v 2 *e*,	v *f*	
	27	v 2 *g*,	v 3 *e*,		Moon 1st. quar.
	28	*g* 1 v,	v 2 *e*,	v 3 *f*	" bright
	30	*g* 1 v,	v 2 *e*,		" full
Feb.	1	*g* 2 v,	*f* 1 v,	v 1 *e*	(naked eye)
		g 1 v,	v 2 *e*		
	8	*g* 1 v,	v 2 *e*		
	10	v 1 *g*,	v 3 *f*		
	11	*g* 2 v,	v 1 *e*		
	12	*g* 2 v,	v 1 *e*		
	14	*g* 3 v,	v *e*		
	16	*g* 1 v,	v 2 *e*		
	17	*g* 2 v,	v 1 *e*		
	20	*e* 2 v,			Moon 7 da.
	22	*e* 2 v,	v 3 *d*		" bright
	24	*e* 3 v,	v 2 *d*		" "
	27	*e* 4 v,	*d* 1 v,	v 1 *c*	
Mar.	5	*c* 4 v,	v *b*		
	7	*c* 2-3 v,	v 4 *b*		
	12	*b* 1 v,			

When not otherwise specified, all these observations were made with the opera-glass.

The first step to be taken in the reduction of these observations is the formation of a light-scale for the comparison-stars; for this purpose we proceed as follows: taking the first observation, we find that *b* is two steps brighter than v, and v four steps brighter than *a*; the observed interval between *b* and *a* is therefore obviously the sum of these two intervals, which is six steps. Proceeding to the next observation, on Jan. 4 we find *d* four steps brighter than v, v equal to *c*, and v four steps brighter than *b*; taking the sum of these intervals then, we find that *d* is four steps brighter than *c*, and eight steps brighter than *b*; *c* is four steps brighter than *b*.

Proceeding in the same manner through the whole series, using always the *sums* of the intervals, and never their *differences*, we get the following table of intervals between the several comparison-stars:

LIGHT-SCALE

b—*a*	*d*—*c*	*d*—*b*	*c*—*b*	*c*—*c*	*f*—*e*	*g*—*e*	*g*—*f*	*e*—*d*
6	4	8	4	6	1	3	2	5
	4		4	5	2	3	1	5
	3		6.5	5	2) 3	3	4	2) 10
	3		3) 14.5	3) 16	1.5	3	3) 7	5.0
	4) 14		4.8	5.3		3	2.3	
	3.5					3		
						3		
						3		
						3		
						3		
						3		
						3		
						12) 36		
						3.0		

Dividing, as shown, the sums of the several columns of observed intervals by the respective numbers of values composing them, we get the following series of mean intervals:

b—*a*	*d*—*c*	*d*—*b*	*c*—*b*	*e*—*c*	*f*—*e*	*g*—*e*	*g*—*f*	*e*—*d*
6	3.5	8	4.8	5.3	1.5	3.0	2.3	5.0

Upon inspecting these means, we find that the star *a* is the faintest of the comparison-stars, and also find from the observations that, in this series, the variable has not been seen fainter than this star, so that it may be given a numerical value of 0.0, and used as the zero-point for the light-scale. It may be here remarked that it is always desirable to set the value for the starting-point of the light-scale at such a figure that there may be no negative quantities among the reduced values.

Next, assemble these intervals as follows, using the numbers of comparisons in each column from which they are formed as the weights for the light-values deduced by their combination. These weights are applied as shown below.

$a = 0$
b is 6.0 steps $> a$ — therefore $b = a + 6.0 = 6.0$
c is 4.8 steps $> b$ — therefore $c = b + 4.8 = 10.8$
d is 3.5 steps $> c$ — therefore $d = c + 3.5 = 14.3 \times 4 = 57.2$
8.0 steps $< b$ $= b + 8.0 = 14.0 \times 1 = 14.0$

$5 \;)\; 71.2$

so that $d = 14.2$

Following this method of assembling, we have

	M		
$e = c + 5.3$	3		
$= d + 5.0$	2	so that $e = 16.5$	So that we have as our light-scale, $a = 0.0$
$f = e + 1.5$	2	$f = 18.0$	$b = 6.0$
$g = e + 3.0$	12		$c = 10.8$
$= f + 2.3$	3	$g = 19.7$	$d = 14.2$
			$e = 16.5$
			$f = 18.0$
			$g = 19.7$

To obtain the light-values indicated by the observations, on the above scale, the method of proceeding is as follows: the first observation on the list is

1895 Dec. 31 *b* 2 v, v 4 *a*

This of course indicates that v was observed on that date to be two steps fainter than *b*, and four steps brighter than *a*. We therefore find from the light-scale what is the numerical meaning of these intervals, and their mean is the light-value sought.

Now the value of *b* on the scale is 6.0, so that *b* 2 v means 6.0 — 2 which is 4.0; the value of *a* is 0.0, so that v 4 *a* means 0.0 + 4.0, which is also 4.0; the indicated values from both stars

being identical, we set down the light value as 4.0.

The next observation is

1896 Jan. 4 *d* 4 v. v *c*, v 4 *b*

The values of these stars being as given by the scale, we have as above;

	d 4 v =	10.2	
	v *c* =	10.8	
	v 4 *b* =	10.0	
divided by the number	3)	31.0	
of comparisons	gives	10.3	which is the light-value sought.

Instead of using the simple or "brute" mean, as above, a refinement is introduced into the reductions by many observers, by the use of weights for the individual comparisons, so that their presumed comparative accuracy may have influence in forming the mean.

It is considered that the comparisons most likely to be carefully studied and decided upon are those showing the equality of the variable with its comparison-star, so that to such comparisons should be assigned the highest weight; for each step of interval this weight is supposed to be less. From the nature of the case, any such scheme of weight is an arbitrary one. The writer has used for many years a system of such weights suggested to him by a very experienced observer; the weight of a comparison showing equality is assumed as 8, so that when the interval shall be as much as four or five steps the weight shall still be sufficient to give it its fair share of influence in forming the mean. For each step of interval, the weight is reduced by one.

On this scheme, the weighted mean for the above observation will be made as follows;

d 4 v = 10.2 × (8−4) = 10.2 × 4 =			40.8
v *c* = 10.8		× 8 =	86.4
v 4 *b* = 10.0 × (8−4) = 10.0 × 4 =			40.0
Dividing the sum of the last column by the sum of the weights, we have		16	)167.2

$$\frac{167.2}{16.} = 10.45$$

The difference is small, not enough to be perceptible in observation, but in reducing long series of observations, the use of a system of weights tends to diminish the influence of discordant comparisons, and to smooth out final results.

In the subjoined table of reduced step-values, the simple means are given, as they are rarely found to vary from the weighted ones by a tenth of a step.

1895 Dec.	31	4.0	1896 Feb.	10	20.8
1896 Jan.	4	10.3		11	17.6
	5	11.5		12	17.6
	6	13.2		14	16.6
	11	13 0		16	18.6
	16	17.7		17	17.6
	22	18.4		20	14.5
	27	20.6		22	15.8
	38	19.4		24	14.8
	30	18.6		27	12.5
Feb.	1	17.4	Mar.	5	6.4
		18.6		7	9.3
	8	18.6		12	5.0

These step-values, when obtained are to be plotted to a convenient scale upon squared paper such as is used for computation, the horizontal distances, or abscissae representing the time, and the vertical. or ordinates the light-values. See figure on page 542

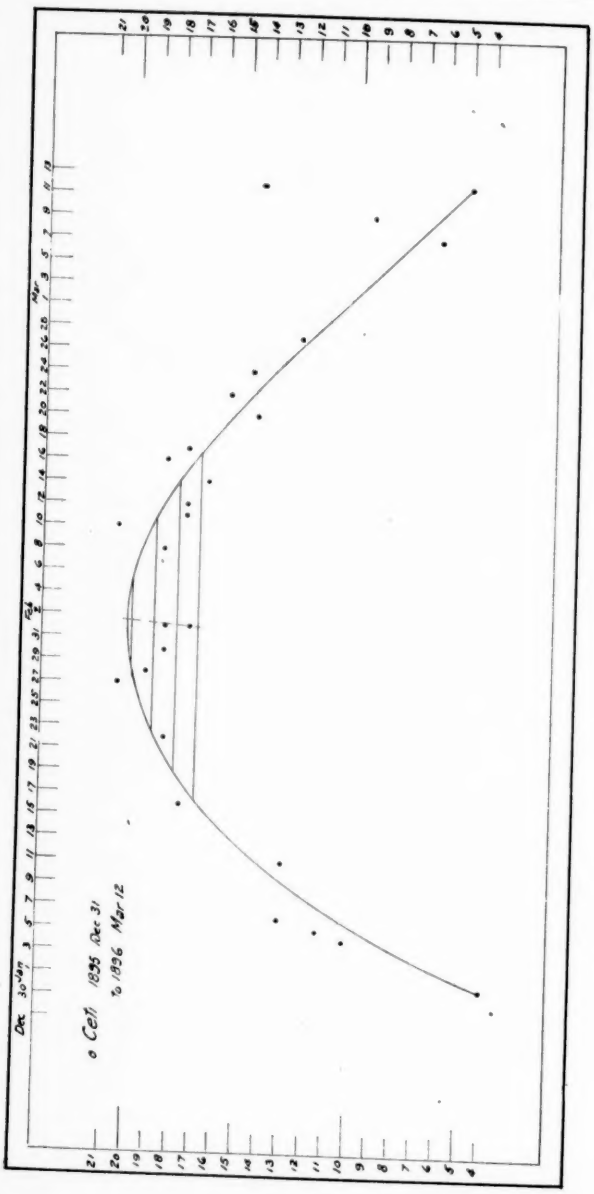
When the points representing these step-values have been plotted, a curve should be drawn through or near them, taking care to avoid abrupt flexures of line, and trying to keep as near to the plotted points as is possible consistently with the above precaution. There should be about the same number of points left on one side of the curve as on the other.

When the curve is drawn, a few lines should be drawn horizontally across it near the highest part, but covering height enough to certainly define the direction of the diameter to be drawn through their central points, whose intersection with the curve indicates the most probable date of maximum.

This line is generally a straight line, but sometimes is curved; this however is immaterial, and the intersection determines the date more independently and impartially than could be done by simple inspection, as practiced by some observers.

In reducing observations of stars of the short-period and Algol types, the same processes are used; but, especially with the stars whose period is less than ten days, it often happens that the observations are so interrupted by moonlight and weather that many of them are so placed as not to yield definite indications of phases by the graphic process here shown; in such cases, recourse is had to a mean light curve, the methods of forming and using which will form the subject for the next paper.

Dorchester, Mass., October 6, 1906.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ON SEISMIC MOTION AND SOME RELATIONS OF EARTHQUAKES TO OTHER PHENOMENA.*

F. OMORI.

Earthquake.—An earthquake consists, as the name signifies, in the trembling of the ground, and may be defined as vibrations or wave-movements propagated through rocks and soil, the motion diminishing with the increase of the distance from the source of disturbance. The magnitude or energy of an earthquake, taken as a whole, may be represented by the area within which the motion is felt; while the intensity of motion at a given place, which depends on the size of the earthquake, decreases in an inverse proportion to the distance. Thus, according to the depth of the earthquake center, there may be large disturbances in which the motion at the surface is not extremely violent, as well as small ones, in which the motion is quite severe.

Disturbance of Waters.—When the earthquake motion of inland origin is large and violent, the waters of ponds, rivers, or lakes are more or less disturbed. So, similarly, a great submarine earthquake is often followed by tidal waves; the time interval between the occurrence of the earthquake shock and the arrival of the sea-waves depending on the depth of the water and the distance of the origin from the shore. The tidal waves following the great Japan earthquake of December 23, 1854, which wrecked the Russian frigate "Diana," then at anchor in the harbor of Shimoda, crossed the Pacific, and were recorded by the tide-gauges at San Francisco and other places on this coast of America.

Tidal waves, which are not to be noticed on the high sea, are developed most markedly on indenting bays with shallow waters. Many of the great earthquakes originating off the Pacific coast of Alaska, and Central and South America have been accompanied by large tidal waves. But fortunately this phenomenon, which sometimes causes more damage than the earthquake disturbance itself, has so far not been very active along the coast of the United States. The great earthquake of April 18th last produced distinct but very small disturbances of the bay waters, which were clearly recorded by the tide-gauge at the Presidio. An examination of the marigrams at several stations on the Pacific Coast of Japan and different places in India has shown that different parts of sea-coast have their proper period or periods of waves,—that is to say, each particular portion of sea-coast is

* Read June 23, 1906, before an adjourned meeting of the Society. Abstracted by A. O. LEUSCHNER.

virtually a fluid pendulum whose boundaries are determined by the form of the bottom and the contour of the shore-line. Accordingly, the wave period or periods at a given coast place remain constant in all tidal waves, irrespective of the origin or cause, a destructive tidal wave consisting simply in the amplification of the wave motion *existing more or less at all times*, in consequence of a strong submarine earthquake, a storm, or some other agency. A seismic tidal wave is caused by the movements communicated from the sea-bottom to the water mass, a very large wave disturbance taking place when the earthquake focus is at the sea-bottom itself or at a very small depth below it.

Dependence of Earthquake Motion on Nature of the Ground.— As an earthquake consists in the vibration of the ground, and the range of motion, or amplitude, of an Earth particle is greater in a soft than in a hard medium, it is evident that the intensity must depend much on the nature of the ground, often differing considerably even within a limited area. The seismic damage is always very slight on hard, rocky ground, and severest on soft, incoherent soil, and especially on newly made ground. From this fact it will be seen at once that the foundation-making plays a most important part in the earthquake-proof construction of buildings and engineering structures. By making the foundation large and solid we approach the condition of a rocky ground. If the foundation be, on the other hand, small and weak, the structure resting on it will be virtually a group of different bodies loosely bound together, and will be thrown into large movements, causing a mutual destruction. As the length of the elastic or proper earthquake waves constituting a destructive shock must be about two or three miles, the slight curvature into which the ground surface is thrown on such occasions will not be evident to the eye. It seems, however, that in soft, marshy soil a violent earthquake sometimes produces, as a secondary phenomenon, a sort of semi-gravity vibration of short wave-lengths, say about twenty or thirty yards. The latter motion, which will be visible to direct observation, and which may be called "visible surface motion," often throws the ground into remarkable curved forms and is very dangerous to structures. But injurious effects of this sort can be entirely avoided by making the foundation of a structure sufficiently large and solid, thereby reflecting back the disturbance consisting of short wave-length vibrations. The case is analogous to the effect of water-waves on bodies floating on the surface. Thus a small fragment of wood or straw behaves as if it were a particle of the water and moves together with the

small ripples; but a large vessel at anchor in a harbor will not be affected by the waves on account of its mass and size.

It may be noted that high ground is, except sand-dunes, generally hard, and therefore good as the site for buildings. But a steep slope or the vicinity of a cliff must be avoided, as the earthquake motion is in such places considerably augmented, owing to the absence of support on the side.

Tremors and Pulsatory Oscillations.—We are accustomed to regard the Earth as solid and firm. But it is a great mistake to suppose that the ground is at rest when we do not *feel* an earthquake. As the Earth's crust is an elastic body, we must assume, in a general way, that it is always making some movements. And so it does in reality, there being besides the tilting or inclination of the ground-level three principal sorts of vibratory movements as follows: (1) Tremors of artificial origin; (2) Small, slow motions, called pulsatory oscillations; (3) Earthquakes.

Among the insensible tremors of an artificial origin I may mention those due to the working of dynamos, steam-engines, steam-hammers, etc. The motion produced by such causes is extremely small, and is generally, except in the immediate vicinity, insensible. It is transmitted, however, sometimes to a distance of half a mile or more, and sets different objects in unstable conditions, so to speak, spontaneously in motion. Thus windows and doors are caused to rattle, bottles on tables to shake, suspended articles to swing, etc., becoming a source of mystery to people in the neighborhood and even giving rise to rumors of ghosts.

Causes of Earthquakes.—The ultimate causes of great earthquakes are probably to be traced to the cooling and contraction of the Earth, and in some degree to the change of distribution of the matter constituting the land and ocean-bottom. The more immediate cause of such earthquakes is, however, frequently due to the activity of mountain-making forces which produce folding or fracturing along extended zones; and any sudden disturbance in the Earth's crust, such as the splitting asunder, fracturing, or falling down of subterranean rock masses, may become the source of earthquake motion. Different external agencies which act on the Earth, and many of which are periodic, may be regarded as secondary causes of earthquake. Thus it will be seen that seismic phenomena, which are themselves periodic in nature, must have certain time and space relations.

After-Shocks.—Numerous small shocks invariably follow a great earthquake. When the latter is violent and destructive the number of these after-shocks may amount to hundreds, or even thou-

sands, and continue for several months or several years. The occurrence of after-shocks is quite natural and necessary for the settling down into stable condition of the disturbed tract at or near the origin of the initial earthquake. Now, the mean time variation of after-shocks follows a very simple law, and analytically may be represented by means of a rectangular hyperbola. In the case of the great Japanese earthquakes of 1891, I calculated an empirical formula from the number of after-shocks observed during the first five days after the initial disturbance, and was enabled thereby to predict the general course of subsequent phenomena, such as the number of years during which these shocks should continue to happen, the total number of after-shocks during a certain year. Similar calculations have been made, with equally satisfactory results, for the after-shocks of other recent Japanese earthquakes. Examples like these show that earthquake phenomena, though apparently mysterious, are not always so very complicated and arbitrary. In the case of the California earthquake of April 18th last, the after-shocks seem to be comparatively very few for some reason or other. These minor shakings, which will continue to happen at intervals for a few years to come, are of course harmless in nature.

Time relations of earthquakes may be distinguished as periodic and semi-periodic. Among the latter class may be included destructive earthquakes which tend to occur in groups,—that is to say, to happen in different regions of a given earthquake zone in the course of a few years. Then there follows a period of rest, after which the seismic activity again commences.

The most well-marked among the periodic seismic variations are those relating to the position of the Sun and the Moon. Thus there are annual variations in the number of earthquakes,—namely, earthquakes occur more frequently in certain months of the year than at others. So again there are more earthquakes during certain hours of the day than during others. The direct principal cause for these variations is, however, not the attraction of the Sun, but the changes in the pressure of the atmosphere. This is in reality the connection between the weather and earthquakes, although the so-called "earthquake weather," or a warm or moist day, is by no means a sure precursor of tremblings of the ground. On the contrary, the high barometric pressure corresponding to fair weather seems to bring more earthquakes when the latter are not of a submarine origin. Again, the study of after-shocks has shown clearly the existence of other periods whose lengths are respectively twelve, eight, six, and four hours,

and also those whose periods are four or five days, eight or nine days, twelve days, etc.

With respect to the lunar influence, there are certain relative positions of the Earth and the Moon at which more earthquakes take place than at other positions. This effect is again not due to the direct attraction of the Moon, but to the variation of the weight of water in the tidal movements. The annual, diurnal, lunar, and some other relations of earthquakes must be treated separately for each given district so as to bring out the local peculiarity.

The attraction of the planets on the Earth will have no appreciable effect on earthquake phenomena.

Latitude Variation.—Apart from the earthquake movements, pulsatory oscillations, and slight changes in the ground-level, the axis of rotation of the Earth is continuously shifting its position through a very small angle, giving rise to the variation of latitude. Professor Milne and the late Dr. Cancain were the first to examine the relations between this phenomenon and great earthquakes, the conclusion reached being that a greater number of the latter occurred in those years in which the variation of latitude was greatest. From an examination of the mean monthly values of the latitude of Tokyo, I have found that all the destructive earthquakes of recent years in Japan occurred exactly or very nearly when the latitude was at a maximum or minimum.

Magnetic Disturbances.—As the magnetic property of rocks and metals varies with the strains to which they are subjected, a great earthquake, which means the removal of an enormous stress in the Earth's crust, may be supposed to be preceded or accompanied by some disturbances in the terrestrial magnetism, at least when the depth of the seismic origin is small. The relation in question has not yet been satisfactorily investigated, but in some cases the terrestrial magnetism which had been quiet for a long time became markedly disturbed a few days before a large earthquake. In this connection, it is interesting to note that the great eruption in Martinique was accompanied by a well-defined magnetic disturbance.

Geographical Distribution of Earthquakes.—In the first place, a country or district which has many volcanoes is one where the Earth's crust is weak, and consequently is often disturbed by underground convulsions or earthquakes. It does not follow, however, that earthquakes are most frequent around a volcano. On the contrary, the immediate vicinity of a great active volcano

is sometimes free from very violent seismic disturbances. Broadly speaking, strong or extensive earthquakes most frequently happen along the region adjacent to the steep sides of a great mountain range or of a series of islands. The variation of gravity in an earthquake country may also have something to do with earthquake geography, and there are cases in which destructive shocks have occurred in the district of a minimum gravity force.

Future studies in various phenomena connected with the movements of the Earth's crust may perhaps tend to advance our knowledge respecting the problem of the prediction of great earthquakes which are often preceded by what may be called "fore-shocks." In the meantime and always it will be necessary to build houses and other structures strong enough to resist earthquake shocks.

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STUDYING THE CORONA OF THE SUN.*

H. H. TURNER.

My special object will be to shew how a *quantitative measure* helps us in studying natural phenomena. The point is not a new one—it is as old as science itself; but every new illustration of it comes with a certain freshness. A beautiful illustration was given nearly thirty years ago by Professor Schuster in almost exactly the same domain as that of which I am about to speak. He attacked† the problem of the distribution of particles round the Sun which might, either by scattering the Sun's light, or by themselves becoming incandescent, give rise to an appearance such as the corona, and wrote thus:—

"Our problem is an inverse one, and seems at first sight very hopeless. From the observed polarization of light we are to find out what part of it is due to scattering particles, and, as will be seen, we cannot do this without finding out at the same time in what way the scattering particles are distributed round the Sun, and in what way the light due to the other causes varies with the distance from the Sun. I began the calculation in the hope of getting a rough idea only of the amount of polarization which we might expect. But it appeared that even such observations as we can make during the short time available during

* The above extract is from a lecture by H. H. Turner before the Literary and Philosophical Society of Manchester England, Manchester Memoirs May 20, 1906.

† *Mon. Not. Roy. Astron. Soc.*, vol. 40, p. 35.

a total solar eclipse may yield most important information as to the constitution of the solar corona. I shall shew that combined measurements (*a*) of the polarization at different distances from the Sun, and (*b*) of the decrease in intensity of the total light of the corona with increasing distance from the Sun will be sufficient to determine all our unknown quantities. Even if such measurements are incomplete, we may gain a rough idea of these quantities, and even a solitary observation like that of Mr. Winter during the eclipse of 1871 will give some results."

In an elegant mathematical investigation, complete in all details, Professor Schuster then proceeds to demonstrate these propositions. But the measurements requisite to utilize his results did not immediately follow, probably because the difficulties of obtaining them by visual methods were too great. The gradual introduction of photographic methods, and especially the invention of the "dry-plate," has made them much easier, and at recent eclipses photographs have been obtained which will probably, when suitably measured, give the required information. As yet, however, measures of the polarization (marked [*a*] above) have not been carried out, and the complete problem formulated by Professor Schuster has not yet been solved. But extensive measures of class (*b*), the total brightness of the corona at different distances from the Sun, were made on photographs taken in 1893* and 1898† (others have been made but are not yet published), and an approximate law was deduced as follows:

The brightness of the Corona is inversely proportional to the sixth power of the distance from the Sun's center.

Now, making use of Professor Schuster's paper above referred to, we can immediately deduce from this that the particles must be distributed according to the inverse 4½ power of the distance and I propose now to examine a little more in detail what further information this gives us about them.

But to prevent misconception, I must recur to one or two points which have been omitted so far in order to simplify the statement. When we come to consider the nature of the Corona, we must remember that it is certainly a complex structure. The spectroscope gives us evidence of the existence of gases which show bright lines in the spectrum. One of these, at least, is a gas unknown to us on Earth, which has been called coronium. But besides gases, there are in the Corona solid, or perhaps liquid, particles; for part of the light which comes to us is polarized

* *Proc. Roy. Soc.*, vol. 66, p. 403.

† *Proc. Roy. Soc.*, vol. 68, p. 36.

light, and from the character of the polarization we can infer the existence of particles of this kind. Moreover we learn something of their approximate size; many of them cannot be very much bigger than a wave length of light, or there would be no such effect. Of course, there may be larger particles than these in the Corona, but these will not polarize the light, and therefore do but dilute the effect. If we find that the polarization is strong, this is evidence that the number of large particles is small. We see here how important it is to get an actual measure of the amount of polarization, and measurements of this kind are in progress. We do know, however, that the polarization is strong, and therefore the small particles numerous, and for the present we are going to consider them alone, as if they constituted the whole Corona. We can afterwards make allowance for the elements neglected.

But the particles are almost certainly not stationary relatively to the Sun; they are either rising or falling, or rising *and* falling: and it is into their state of motion that I propose to inquire, in the light of the measurement above quoted.

Let me first take perhaps the simplest case. We have said that the particles must be small. Now Clerk-Maxwell pointed out years ago how the radiation of light would exert a *pressure* on bodies receiving the light, which would be quite insensible for large bodies but might become important for very small ones. This remark has scarcely received proper attention until recently, but in the last year or two Professor Poynting* and others have stated in definite form the amount of this light pressure, and shown that for bodies smaller than two wave lengths of light the pressure may be so great as to counterbalance the Sun's gravitational attraction. If so, the particles *may* be continually repelled from the Sun, instead of attracted. Repulsion could, of course, also follow from electrical action of the Sun. For our purpose these two hypotheses can be considered together. Supposing, then, particles to be *repelled* from the Sun, how far would their distribution fit in with the observed law of brightness? To a certain extent, the supposition looks promising; starting from the surface, where the particles may be densely packed, they are spread over a larger and larger surface as they travel outwards. Moreover, since they are traveling with ever-increasing velocity, their density will further diminish owing to this fact. Further, the light received by each diminishes with increasing distance

* *Phil. Trans.*, Series A, vol. 202, pp. 525-552; and *Proc. Physical Society of London* vol. 19; also *Phil. Mag.*, April, 1905.

from the Sun whether the light be simply scattered sunlight or the incandescent light of the particle itself, heated up as it must be by the intense radiation from the Sun: but this is taken account of in Professor Schuster's paper and the deduction we made from it, whereby we converted the law of brightness into a law of density of particles, viz., as the inverse 4½ power of the distance. Combining, then, for comparison with this observed density the two former contributing causes—greater surface, which goes as the square, and greater velocity, which is as the square-root at most—we get only 2½ instead of 4½: and our supposition does not fit the observed facts. Hence, if particles traveling outwards (under repulsive force) exist in the corona, they can only *dilute* the effect we wish to explain, and we must look for other particles moving in such a way that the index is even greater than 4½, so that it may be reduced to 4½ on the average: just as when, in climbing a hill of which the average slope is known, if we find an easy gradient for some distance, we know that we shall ultimately find an unusually steep part somewhere.

Next let us take another supposition of an inverse kind. Matter cannot be traveling continuously outwards. Can it be, traveling continually inwards from space? This supposition is worse than the former; the condensation as we approach the Sun being not even so great as before. We still have the "concentration" due to the decrease in surface of concentric spheres, but when we come to the velocity with which a particle crosses any sphere, it is now *greater* near the Sun instead of *less*, as before. Accordingly our power of the distance is no longer even so large as 2½, but on the supposition of simple falling, it would be only 1½. There is, however, one new consideration which may be taken into account. Is there anything resisting the falling? We know, for instance, that there is in the Corona a gaseous portion made up of hydrogen, coronium, and perhaps other gases. Do these check the action of the falling particles? When the great eruption of Krakatoa took place twenty years ago, a large amount of dust was flung sky high by the volcano, which took years to fall. Many of us remember the beautiful sunset glows of the years near 1884, which were probably due to the existence of this dust high up in our atmosphere, and Professor Stokes showed how the observed rate of fall of these particles was just such as might be inferred from observations of the way in which they scattered light. But if I understand his formula rightly, the rate of fall would be nearly uniform at all heights.

It would not at any rate diminish as the particles approach the Earth at a rate so marked as the cube of the distance, which is the kind of change we require. We must, of course, be cautious in arguing from our own atmosphere to that of the Sun, but in default of positive knowledge to the contrary, I put this supposition aside as not helpful for the illustration of our present problem. [It seems, however, just possible that the resistance of the gaseous atmosphere, combined with a resultant force *outwards*, might satisfy the conditions. This consideration occurred to me too late for full examination.]

Let us, then, turn to some other supposition. Light pressure or electrical repulsion must not overcome gravity, but they may considerably reduce it. One result of this reduction is that a much smaller velocity is necessary to carry a particle to a given height from the Sun. If gravity were not so reduced, a velocity of 382 miles per second would be necessary to eject a particle completely from the Sun so that it never returns, and a velocity not much less is required to eject it to heights such as are represented in the corona. For instance, to reach a height of one solar diameter the velocity must be 270 miles per second. There is nothing impossible or even unlikely in the existence of such velocities at the Sun's surface *a priori*, though presently we shall see some reason for doubting their existence. But for the moment let us leave on one side the consideration of the absolute magnitude of these velocities, only remarking a diminution of the Sun's attraction is for our purpose equivalent to a diminution in the velocity of projection. We can substitute one for the other and leave the path of the particle practically unaltered; so that we lose no generality by asking how particles ejected with different velocities and in different directions would be distributed, keeping their size and the Sun's attraction the same. For simplicity let us restrict the problem further still and consider the variation in magnitude and direction separately. Let us first suppose a velocity given in magnitude but varying in direction. Take, for instance, a velocity, just sufficient to project a particle vertically to the height of one radius of the Sun; then the same velocity, with horizontal projection, would send the particle skimming round the Sun completely as a very close satellite; while intermediate directions would cause it to describe trajectories of different heights; all, however, less than one radius. If particles be shot out from a point on the surface in all directions with this velocity, there will certainly be more of them at any moment near the surface than far away. Beyond one radius there are none at all,

and just within that limit there are only a few projected nearly vertically. Lower down we get a greater number from directions more and more inclined to the vertical, and so as we approach the Sun the density increases. Close to the Sun, indeed, it becomes infinite; for the particles which come skimming along the surface occupy that region for a finite time, whereas all other particles are at any particular distance for an infinitesimal time only. Working out the law of density mathematically, we find that it is not unlike that required, excluding the little shell close to the surface which may be regarded as part of the chromosphere. But the early promise of this supposition is not followed up. Firstly, it would only give us a corona one radius deep and the observed corona is much bigger than this; accordingly we must increase our possible velocity so that particles may rise several radii from the Sun, and on coming to this more general case we find quite a different law of distribution. Let us take, for example, a velocity which would carry a particle to the height of three radii vertically. If particles were spouted from a point on the Sun's surface in all directions with this velocity, the density at the surface of the Sun comes out infinite as before; at a little distance it is finite but decreasing very slowly. The decrease ultimately stops, and then becomes an increase; and we get *another shell with infinite density*, due to the fact that in the neighborhood of a point on the other side of the Sun from the eruptive center there is a great accumulation of particles in a part of their orbits where they remain for some time at nearly the same solar level. Outside the shell the density falls off at the rate we desire, but fails altogether at a height of three radii. This supposition accordingly is totally at variance with the observed facts and we are led to the conclusion that variations in direction of projection apart from variations in amount do not give us an adequate explanation of the corona.

We must fall back on variations in magnitude of velocity. But the work already done has not been thrown away; it has simplified the problem. We have seen that with a given magnitude of velocity there is a certain shell where the density becomes infinite compared with that inside and outside; and hence in adding together the result of velocities differing in magnitude, we need only take account of this particular shell in each case, and we arrive at a conception of the corona as made up of a series of shells one inside the other, corresponding to different velocities. But this does not accord at all well with its appearance; it would give a stratified corona, whereas the observed corona is distinctly radial in character.

For reasons which would be tedious to give at length, the evidence seems to indicate that variations in *direction* of velocity must be very small, and that we may assume the direction to be approximately vertical. The corona near the Sun is formed from *low* vertical jets; that at a distance, from jets which reach a great height; and since the former is so much denser, there must accordingly be many more velocities of small size than of large. The law of degradation of brightness is accordingly now become a law telling us how many more small velocities there are than large ones, and we accordingly return to what was originally said about the magnitude of these velocities.

It may not really be the velocities themselves which vary, but the force of the Sun's attraction, as diminished by light pressure. When the light pressure and attraction nearly balance one another, a very slight change in the adjustment will double the difference, and it is obvious that we have here great possibilities for variation in magnitude. When we introduce this variation into our equations, we find a notable effect, somewhat of the kind required, on the distance to which the particles are ejected. If we assume a series of attractive forces, in the ratios 10, 9, 8, 7, 6, 5, then a velocity which would, under force 10, eject a particle to the height of 1 radius from the surface (or 2 from the center—for comparison with our law it is better to measure from the center), would, with the forces 9, 8, 7, 6, eject it to 2.2, 2.7, 3.5, 6.0 radii from the center; while under force 5, the particle would leave the Sun altogether. The scattering of the particles is thus very rapid at distances greater than two radii from the center. But the same does not hold *within* this distance. If we extend the series of forces in the other direction by the numbers 11, 12, 13, 14, 15, the corresponding distances from the center are 1.83, 1.71, 1.63, 1.56, 1.50: in other words, the inner corona would be nearly uniform, unless we have some other source of variation. This we must seek in the distribution of the sizes of the particles. We must suppose that those which rise to considerable heights, because light pressure nearly balances the Sun's attraction, are comparatively few, while there would be many for which the balance was less complete.

We have, therefore, to a certain extent, only replaced one difficulty by another—or rather, only one inquiry by another. Instead of looking for a cause for a certain distribution of density, we now seek the reason of a certain distribution of size. But I think that we have advanced a step, although we may not have completely solved the problem. That there should be this varia-

tion in size near the point where light pressure nearly balances attraction is of the nature of a *vera causa*.

At the risk of appearing to argue in a circle, I now call attention to the effect of this supposition on our estimate of the absolute magnitudes of the velocities. If we are right in regarding the variations of magnitude as taking place in the force (and not in the velocity of projection), then the average force is small, and the velocities will also be small. This affords us a loophole of escape from a possible difficulty. If particles were ejected with velocities of this magnitude, we might reasonably expect the corona to change its form, at any rate in detail, within a very short time. In one hour, for instance, a particle would travel over a million miles—more than a solar diameter; and though a jet may retain the same general appearance, though composed of entirely new matter, we might hope to detect differences of detail in it. Good snapshots of Niagara, for instance, differ in detail, however quickly they may succeed each other. Now pictures of the corona at an interval of one hour do not differ in this way, and though we must not lay too much stress on the evidence as yet, it affords a presumption in favor of lower velocities. Hence it is a point gained that light-pressure enables us to look for lower velocities of ejection. For instance, instead of the 382 miles per second necessary to eject a particle completely from the Sun, one-tenth of this velocity only is required if the force be 100 times smaller, so that we may now contemplate velocities of ten or twenty miles per second, which would with such force raise a particle to a height of several radii, as taking part in the phenomenon. Now it is interesting to find that we have indications of velocities of this size from a quite independent research, namely, that on the solar granules. The surface of the Sun in a telescope presents a mottled appearance. Forty years ago, Mr. James Nasmyth, in a letter to a member of this society—of which within a few months he became a corresponding member—claimed that he had discovered as an explanation of this mottled appearance, that there were scattered over the Sun's surface a number of objects resembling "willow leaves." Thereupon ensued a curious scientific controversy. Other observers saw the phenomenon indeed which Mr. Nasmyth intended to describe, but found fault with his description. Some preferred the name "rice-grains" for the objects which they admitted were there; others spoke of bits of straw. Their various ideas may be illustrated on the screen. Attention was after some years distracted from this controversy by the work of the spectroscope, but within the last year our in-

terest has been reawakened in it by some very successful photographs* taken by Mr. Hansky, of St. Petersburg, who has not only photographed these granules, but demonstrated their continuous existence as separate bodies of some kind, moving about among one another. But to identify them as individuals it is necessary to take photographs at intervals of less than one minute, for the movements even in a few seconds are large. By measuring the photographs, Mr. Hansky estimates that the velocities of the granules are something like 10 to 20 miles per second. If we may assume that there are in a vertical direction velocities similar to these horizontal ones, we get just the sort of velocity which would suit the phenomena of the corona on the lines above indicated. I do not lay, however, too much stress on this illustration, if only for the reason that Mr. Hansky's discovery is too recent. But I would add one word further of what may be frankly called speculation in the direction of very low velocities.

If for any reason it were to seem probable that velocities even smaller than these might play a part in the phenomenon of the corona, velocities, let us say, of one mile per second, then we must not forget to take into account the possible effect of the Sun's rotation. Supposing, for a moment, that the particles owed their ejection to this rotation alone, we can see that they would be chiefly flung off from the equator and the corona would have the appearance of a comparatively flat extension, which we see at a time of minimum sunspots. The extension in other directions seen when sunspots are numerous might then be due to the introduction of some disturbing cause in addition to the Sun's rotation. But for considerations of this kind to have any value, the velocities of ejection must be of the same order as that of a particle rotating at the Sun's equator, which is about one mile per second. And for this to be effective in carrying it out to several radii from the Sun, we must suppose the attractive force diminished by light pressure to the 100,000th part of itself —a supposition which has not much to recommend it.

To sum up, then, the result of this discussion:—

The observed falling off in light of the Corona must be referred to the fact that light pressure nearly, but not quite, balances gravity for the particles forming the corona; that the difference accordingly varies in magnitude; that there are many more particles for which this difference is large than for which it is small; and that, without any further considerable diversity in conditions, particles ejected vertically from the Sun's surface with

* *Mitt. der Nikolai-Hauptsternwarte zu Pulkowa*, vol. I, no. 6.

velocities similar to those observed in the granules might then distribute themselves according to the observed law. It must be remembered that we have not taken account at all of the incandescence of the particles, which may vary according to almost any law of distance. We have started from the fact that the corona shows strong polarization—in other words, that scattered light must play, at any rate, a considerable part in the appearance—and limited the inquiry to this part of the received light.

The discussion is therefore partial only, and reaches no final conclusion. But it is, perhaps, the more characteristic of eclipse work. We are only slowly spelling out, with long intervals between each letter, the lessons to be learnt from the Corona; and even those we seem to have mastered we must be ready to modify in the light of new facts.

A 100-INCH MIRROR FOR THE SOLAR OBSERVATORY.

GEORGE E. HALE.

I am permitted to announce that Mr. John D. Hooker, of Los Angeles, has presented to the Carnegie Institution of Washington the sum of forty-five thousand dollars, to be used to purchase for the Solar Observatory a glass disk 100 inches (2.54m) in diameter and 13 inches (33cm) thick, and to meet other expenses incident to the construction of a 100-inch mirror for a reflecting telescope of 50 feet (15.24 m) focal length. These expenses will include the erection of a building in which the mirror can be ground, figured, and tested; the construction of a large grinding-machine, with crane for lifting the mirror; the provision of a 54-inch (1.37 m) glass disk, to be made into a plane mirror for testing purposes; the purchase of glass disks for the various plane and convex mirrors required in the telescope, etc. The optical work will be done by Professor G. W. Ritchey and the assistant opticians employed under his direction by the Solar Observatory.

In making this gift, Mr. Hooker's desire is to secure the realization of the great possibilities in astrophysical research which a large reflector seems to offer. He has absolute confidence in the ability of Professor Ritchey to make an essentially perfect mirror 100 inches in diameter; no one could ask better evidence of this than his gift affords. He knows, also, that in several classes of work, such as the measurement of the heat radiation of the stars, and the spectroscopic study of the faintest objects, the mirror is sure to yield results fully commensurate with its great size. But he is nevertheless aware that for certain other classes of work,

in which the most perfect definition is essential to the highest success, the construction of a mirror of such great aperture must be regarded as an experiment. The immense block of glass will weigh four and one-half tons—four and a half times as much as the disk of our 60-inch (1.52 m) mirror. The difficulty of providing a mounting capable of carrying it with the necessary precision is not slight. The glass is certain to be distorted by temperature changes, which would ruin its performance if not obviated. The atmospheric conditions, even on Mount Wilson, may not be sufficiently good to permit so great an aperture to be used to full advantage. Of these and other obstacles Mr. Hooker is fully informed, and he does not underestimate their importance. But he perceives and appreciates, with the understanding of one who has himself invented and developed mechanical appliances, that experiment is necessary to progress. He therefore does not hesitate to provide the means for undertaking an optical experiment on a large scale. Let us consider its probable outcome.

In the first place, the question arises whether a sufficiently homogeneous glass disk of the required dimensions can be obtained. Our long experience with the Plate Glass Company of St. Gobain leads us to believe that no insuperable difficulty will be encountered. This old and reliable company has cast for us scores of disks, from which Professor Ritchey has made a large number of plane and concave mirrors, from the smallest sizes up to 60 inches. At present the 60-inch is receiving the finishing touches in our optical shop, and two 36-inch (0.91 m) mirrors, for testing purposes, are well advanced, in addition to several large plane and convex mirrors, for the 60-inch reflector. In all of these cases the glass disks furnished by the St. Gobain Company have left nothing to be desired. The 60-inch disk, 8 inches (20.3 cm) thick, and weighing a ton, is fully equal in quality to the smaller ones. We are therefore inclined to believe, since the St. Gobain Company expresses its deliberate opinion that a satisfactory disk, 100 inches in diameter and 13 inches thick, can be produced, that they will be able to carry out the order we have given them.

As for the work of grinding and figuring, no one who has watched the progress of our 60-inch mirror would be likely to doubt Professor Ritchey's ability to accomplish this difficult task. The method of parabolizing which has recently perfected will apply as well to a 100-inch mirror as to the 60-inch. It eliminates the necessity of any hand-work, and has already yielded a paraboloidal figure so perfect that almost any other optician would be more than contented with it. Professor Ritchey rightly believes, however, that a still higher degree of perfection will be worth attaining, since its advantages will be felt under the most perfect atmospheric conditions. I am confident that he will find no difficulty in bringing the 100-inch mirror, as well as the 60-inch, to this highest order of perfection.

The mounting should offer no great obstacles, especially as it will not be designed until the mounting of the 60-inch has been thoroughly tested on Mount Wilson. Unless I am greatly mis-

taken, this latter instrument will meet our best expectations. Professor Ritchey has taken the greatest pains with the design, and the coöperation of the able staff of engineers at the Union Iron Works has been most useful. The mechanical execution of the parts is admirable, and with the heavy machinery available, a mounting much larger than that required for the 100-inch mirror should be easily within the bounds of possibility. To a firm which has built some of the most powerful battleships and crusiers in our navy such a mounting would appear much less formidable than to the average instrument-maker, accustomed to a different class of work. Fortunately, the ideas of the Union Iron Works Company, as to the degree of precision required, are entirely in harmony with our own, and appear to have been met in the mounting of the 60-inch, which has just arrived in Pasadena.

The prevention of change of figure due to changing temperature should not prove a very serious problem. During the fine nights of the best observing season on Mount Wilson the temperature remains almost perfectly constant after 9 p.m. It will therefore only be necessary to maintain the mirror (or possibly the entire telescope) at approximately this temperature throughout the day, by means of suitable refrigerating machinery. In the long periods of absolutely cloudless weather the change of temperature from night to night is extremely small, so that little difficulty should be encountered on this score. If the slowly falling temperature during the early evening should prove to give trouble, the observational work might be deferred until after nine o'clock. The dome and building like those designed for the 60-inch reflector, will be so constructed that no air can enter during the day; they will also be protected by louvers from the heat of the Sun. The problem is, of course, altogether different from that encountered in the case of the Snow telescope, where the mirrors are required to give good images in spite of their exposure to direct sunlight.

Assuming that these various difficulties can be successfully overcome, it still remains a question whether the atmospheric conditions on Mount Wilson will be sufficiently good to permit the telescope to give satisfactory images. This cannot be definitely determined until after the 60-inch reflector has been used for some time. Even if it should prove, however, that only a very few nights in the course of a year can be utilized to the fullest advantage, the construction of such a telescope would nevertheless be desirable. For under the ordinary conditions, which are much finer than those in the eastern part of the United States, results of the highest value can be obtained in many classes of work, such as the photography of stellar spectra, the measurement of the heat radiation of the stars, etc. The immense amount of light which this mirror will collect should render it particularly suitable for spectroscopic work of all kinds.

It need hardly be said that the 100-inch mirror, when suitably mounted, will play a most important part in the scheme of research of the Solar Observatory. The investigation of stellar evolution, upon which we are engaged, frequently calls for adequate spectroscopic study of the stars beyond the reach of

existing instruments. In my work on the red stars of Secchi's fourth type, with the 40-inch Yerkes telescope, I encountered this difficulty, in spite of the great light-collecting power of that instrument. It was impossible to obtain satisfactory evidence as to the transition from solar stars to those of the fourth type. The large number of stars within the reach of a 100-inch reflector should greatly increase the possibility of finding the intermediate types, which are so important in their bearing upon the relationship of solar and red stars. This is only a single instance, but it forcibly suggests itself when considering our program of research. In other fields the large reflector should be equally valuable, especially for the photography of the numerous small spiral nebulae, the details of which should be brought out to good advantage with a focal length of 50 feet; minute study of the large nebulae, in the hope of detecting changes in their form; the study, with very high dispersion, of the spectra of bright stars, etc. The remarkable calm of the summer nights on Mount Wilson should assist materially in all of this work, since vibration of the tube, caused by the wind, would undoubtedly be a serious drawback under less favorable conditions.

No provision has yet been made for the mounting and dome. It is not known from what source funds for this purpose will come, but I believe a donor will be found by the time they are needed. Mr. Hooker's gift is very opportune, because of the fact that it permits us, now that the 60-inch mirror is nearly completed, to retain and use to the best advantage the services of the opticians trained by Professor Ritchey for our present work. The making of the glass disk, and the grinding and figuring of the various mirrors, will probably occupy about four years. Since the Union Iron Works Company will require only a year for the construction of the mounting and dome, it is evident that no funds for this purpose will be needed at present, and that the experience gained with the 60-inch reflector can be utilized in designing them.

PURCHASE OF THE SNOW TELESCOPE BY THE SOLAR OBSERVATORY.

As stated in previous papers, the authorities of the University of Chicago were kind enough to loan the Snow telescope to the Solar Observatory for a period of two years. It subsequently became the opinion of all parties concerned that, in view of the very satisfactory performance of this instrument on Mount Wilson, it would be advisable to keep it there permanently. Accordingly, the Snow telescope has been purchased by the Solar Observatory, and will thus form a part of its permanent equipment. I wish to express my sense of obligation to Miss Snow, to Professor Frost, Director of the Yerkes Observatory, and to Acting President Judson and the Trustees of the University of Chicago, for the courtesies shown us in connection with the loan and sale of this valuable instrument.

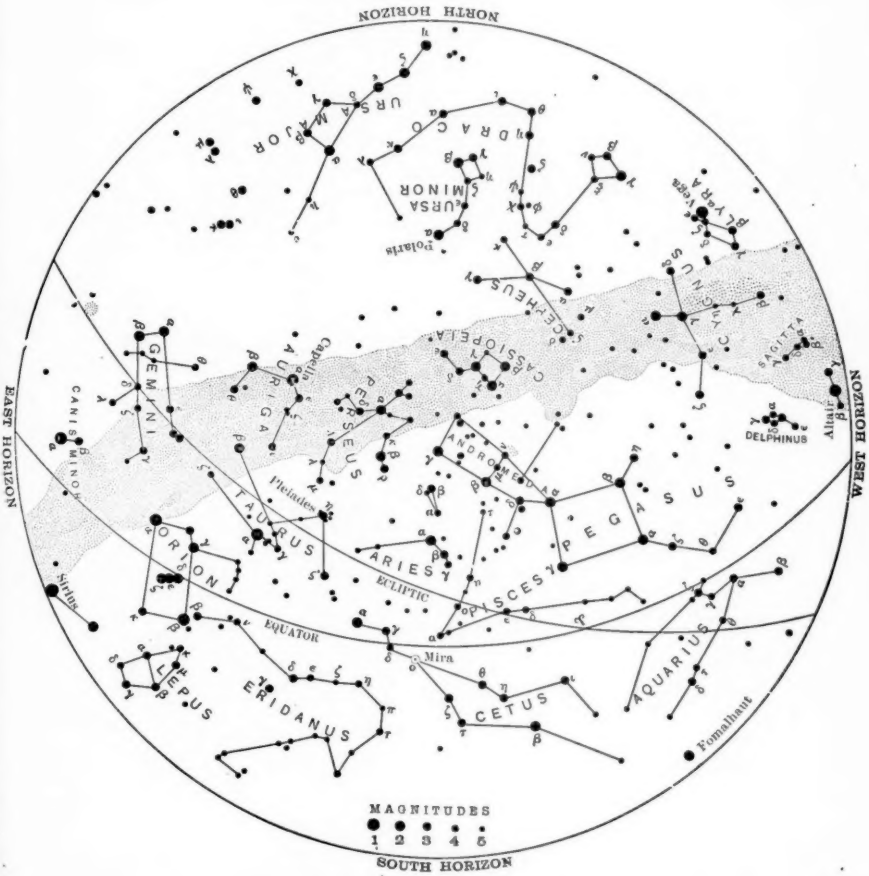
ASTROPHYSICAL JOURNAL.
October, 1906.

EAST HORIZON

PLANET NOTES FOR DECEMBER, 1906.

H. C. WILSON.

Mercury is morning star during December and will be at greatest elongation, west from the Sun 21° 35′, on December 18. It may be seen toward the south-east about an hour before sunrise during the middle of the month. In our note last month we said that Mercury might be visible as evening star during the first half of November. This will be true for observers located on the southern hemisphere of the Earth, but for northern observers Mercury, being farther south than the Sun, will set only a few minutes after sunset.



THE CONSTELLATIONS AT 9:00 P. M., DECEMBER 1, 1906.

Venus being at inferior conjunction November 29 will be invisible during the first days of December. Perhaps by the end of the first week the planet may be caught on the southeast horizon about a half hour before sunrise. She comes out from the glare of the Sun quite rapidly and at the end of the month will rise about three hours before sunrise. Mercury and Venus will be in conjunction on the morning of December 13, Mercury being then only 48′ north of Venus.

Mars is also morning star and is to be found in the constellation Virgo seen toward the southeast after four o'clock. Mars will be in conjunction with Spica on the morning of December 3, the planet being then 3° 25′ north of the star.

Jupiter will be at opposition on the morning of December 28. This planet is now in splendid position for the study of the surface markings, its declination being 23° north and the meridian altitude in our latitude being 67°. It is in the constellation Gemini and is far more brilliant than any of the stars, so that one has no difficulty in identifying the planet.

Saturn is at quadrature, 90° east from the Sun December 1 and so is in good position for study during the early evening hours. The planet is easily found in the constellation Aquarius, being much brighter than any of the stars in that vicinity. The rings are a striking feature of the planet but they are turned nearly edgewise to us so that the details of their structure connot be satisfactorily studied.

Uranus will be in conjunctian with the Sun December 30 and so is invisible.

Neptune is coming near to opposition and so is in favorable position for study during this month. Neptune on December 1 will be about 3° east and 1° south from Jupiter, but can be seen only with the aid of a telescope. With a small telescope it looks exactly like a star of about the eighth magnitude and it can be identified only with the aid of a star chart or by its movement from night to night.

The Satellites of Saturn.

Central Standard Time reckoning from noon.

I. Mimas. Period $0^d\ 22.^h6$

Dec.	h		Dec.	h		Dec.	h		Dec.	h	
1	7.3	E	8	9.0	W	16	9.1	E	24	9.2	W
2	5.9	E	9	7.6	W	17	7.7	E	25	7.8	W
3	4.6	E	10	6.2	W	18	6.3	E	26	6.4	W
			11	4.8	W	19	4.9	E	27	5.0	W

II Enceladus. Period $1^d\ 08.^h9$.

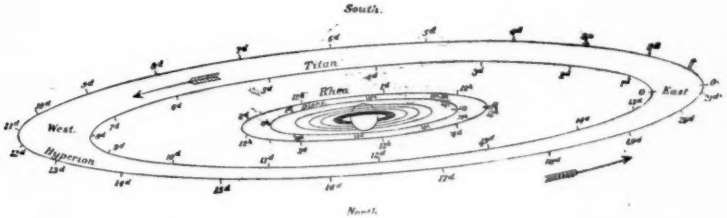
Dec.			Dec.			Dec.			Dec.		
1	1.0	E	9	6.3	E	17	11.7	E	25	17.1	E
2	9.9	E	10	15.2	E	18	20.6	E	27	2.0	E
3	18.7	E	12	0.1	E	20	5.5	E	28	10.9	E
5	3.6	E	13	9.0	E	21	14.4	E	29	19.8	E
6	12.5	E	14	17.9	E	23	23.3	E	31	4.7	E
7	21.4	E	16	2.8	E	24	8.2	E			

III Tethys. Period $1^d\ 21.^h3$

Dec.			Dec.			Dec.			Dec.		
1	2.0	E	8	15.3	E	16	4.6	E	23	17.9	E
2	23.3	E	10	12.6	E	18	1.9	E	25	15.2	E
4	20.6	E	12	9.9	E	19	23.2	E	27	12.5	E
6	17.9	E	14	7.2	E	21	20.5	E	29	9.9	E

IV Dione. Period $2^d\ 17.^h7$.

Dec.			Dec.			Dec.			Dec.		
2	19.1	E	8	6.5	E	16	11.6	E	24	16.8	E
5	12.8	E	11	0.2	E	19	5.3	E	27	10.5	E
			13	17.9	E	21	23.1	E	30	4.2	E



Apparent orbits of the seven inner Satellites of Saturn as seen in an inverting telescope.

V Rhea. Period $4^d\ 12.^h5$.

Dec.	4	5.2	E	Dec.	13	6.2	E	Dec.	22	7.2	E
	8	17.2	E		17	18.7	E		26	19.7	E

Dec.	31	8.2	E								

VI Titan. Period $15^d\ 23.^h3$.

Dec.	1	4.2	E	Dec.	9	5.8	W	Dec.	17	3.5	E
	5	8.0	I		13	1.8	S		21	7.3	I

Dec.	25	5.1	W								
	29	1.1	S								

VII Hyperion. Period $21^d\ 7.^h6$.

Dec.		6.0	I	Dec.		15.4	S	Dec.		21.0	E
		11.0	W								

Dec.		27.3	I								

VIII Iapetus. Period $79^d\ 22.^h1$.

Nov.		4.2	W	Nov.		24.0	S	Dec.		13.4	E

Jan.		2.8	I								

Phenomena of the Satellites of Jupiter.

Central Standard Time, beginning at noon.

Dec.	1	7	32	II Sh. In.	Dec.	9	16	01	III Sh. Eg.
		8	50	II Tr. In.		10	8	56	II Oc. Re.
		10	21	II Sh. Eg.			13	14	I Sh. In.
		11	40	II Tr. Eg.			13	40	I Tr. In.
		16	51	I Sh. In.			15	31	I Sh. Eg.
		17	30	I Tr. In.			15	57	I Tr. Eg.
	2	9	03	III Sh. In.		11	10	24	I Ec. Dis.
		11	34	III Tr. In.			13	04	I Oc. Re.
		12	00	III Sh. Eg.		12	7	42	I Sh. In.
		14	02	I Ec. Dis.			8	06	I Tr. In.
		14	34	III Tr. Eg.			9	59	I Sh. Eg.
		16	54	I Oc. Re.			10	23	I Tr. Eg.
	3	6	41	II Oc. Re.		13	7	19	III Oc. Re.
		11	20	I Sh. In.			7	30	I Oc. Re.
		11	56	I Tr. In.			8	42	IV Sh. In.
		13	36	I Sh. Eg.			9	56	IV Sh. Eg.
		14	13	I Tr. Eg.			12	02	IV Tr. In.
	4	8	30	I Ec. Dis.			13	25	IV Tr. Eg.
		11	20	I Oc. Re.		15	12	44	II Sh. In.
	5	6	22	I Tr. In.			13	22	II Tr. In.
		8	05	I Sh. Eg.			15	33	II Sh. Eg.
		8	39	I Tr. Eg.			16	12	II Tr. Eg.
	6	15	59	II Ec. Dis.		16	17	01	III Sh. In.
	8	10	08	II Sh. In.		17	7	51	II Ec. Dis.
		11	07	II Tr. In.			11	10	II Oc. Re.
		12	57	II Sh. Eg.			15	08	I Sh. In.
		13	57	II Tr. Eg.			15	24	I Tr. In.
	9	13	02	III Sh. In.			17	25	I Sh. Eg.
		14	53	III Tr. In.			17	40	I Tr. Eg.
		15	56	I Ec. Dis.		18	12	19	I Ec. Dis.

Phenomena of the Satellites of Jupiter.—(Continued.)

Dec.	18	14	48	I Oc. Re.	Dec.	26	4	39	II Sh. In.
	19	5	19	II Tr. E.g			4	44	II Tr. In.
		9	36	I Sh. In.			7	28	II Sh. Eg.
		9	49	I Tr. In.			7	34	II Tr. Eg.
		11	53	I Sh. Eg.			11	30	I Sh. In.
		12	6	I Tr. Eg.			11	33	I Tr. In.
	20	6	47	I Ec. Dis.			13	48	I Sh. Eg.
		6	53	III Ec. Dis.			13	50	I Tr. Eg.
		9	14	I Oc. Re.		27	8	40	I Oc. Dis.
		10	35	III Oc. Re.			10	52	III Ec. Dis.
	21	6	22	I Sh. Eg.			10	57	I Oc. Re.
		6	32	I Tr. Eg.			13	50	III Oc. Re.
		17	14	IV Ec. Dis.		28	5	58	I Tr. In.
		18	31	IV Ec. Re.			5	59	I Sh. In.
	22	15	20	II Sh. In.			8	16	I Sh. Eg.
		15	37	II Tr. In.			8	16	I Tr. Eg.
	24	10	26	II Ec. Dis.		29	5	23	I Oc. Re.
		13	24	II Oc. Re.		30	3	35	IV Tr. Eg.
		17	02	I Sh. In.			4	13	IV Sh. Eg.
		17	07	I Tr. In.		31	12	44	II Oc. Dis.
	25	14	13	I Ec. Dis.			15	45	II Ec. Re.
		16	31	I Oc. Re.					

Note.—In., denotes ingress; Eg., egress; Dis., disappearance; Ec., eclipse; Oc., occultation; Tr., transit of the satellite; Sh., transit of the shadow.

Occultations Visible at Washington.

Date 1906	Star's Name	Magnitude.	IMMERSION. Washington M.T. h	m	Angle f'm N pt. °	EMERSION. Washington M.T. h	m	Angle f'm N pt. °	Duration. h	m
Dec. 2	15 Geminorum	6.5	12	58	25	13	46	325	0	48
2	16 Geminorum	6.2	12	40	105	14	10	247	1	30
3	56 Geminorum	5.2	14	30	90	16	02	283	1	32
3	61 Geminorum	5.8	17	58	88	19	07	295	1	09
21	B. A. C.8214	6.5	3	11	27	4	16	280	1	05
27	63 Tauri	5.7	6	09	29	7	08	286	0	59
28	*m* Tauri	5.0	3	45	6	4	04	325	0	19
29	χ^4 Orionis	4.7	6	25	71	7	32	263	1	07
30	ζ Geminorum	Var.	12	01	110	13	31	255	1	30
31	85 Geminorum	5.2	11	46	95	13	18	278	1	32
31	BD.+20°, 1976	6.3	15	36	56	16	27	333	0	51

Phases of the Moon.

Central Standard Time.

		h	m
Last Quarter	Dec.	8	7 45
New Moon		15	0 54
First Quarter		21	21 03
Full Moon		30	0 44

COMET AND ASTEROID NOTES.

Ephemeris of Comet Finlay (1906 *d*).

Paris Midn.

1906		α app. h	m	s	δ app. °	′	″	log Δ	Aberr. m	s	Br.
Nov.	1	8	22	8.08	+21	15	48.8	9.81325	5	24	
	2		22	56.28	21	18	25.8	81468		25	1.50
	3		23	40.95	21	21	11.7	81606		26	
	4	8	24	22.08	+21	24	6.8	9.81738	5	27	1.44

1906		α app.		δ app.			log Δ	Aberr.	Br.
	h	m	s	°	′	″		m s	
Nov. 5	8	24	59.63	+21	27	11.4	9.81865	5 28	
6		25	33.57	21	30	25.8	81988	29	1.39
7		26	3.88	21	33	50.2	82106	30	
8		26	30.52	21	37	24.8	82220	31	1.34
9		26	53.47	21	41	9.8	82330	32	
10		27	12.69	21	45	5.4	82437	33	1.29
11		27	28.17	21	49	11.7	82541	33	
12		27	39.87	21	53	28.9	82642	34	1.24
13		27	47.78	21	57	57.0	82740	35	
14		27	51.88	22	2	36.0	82837	36	1.20
15		27	52.15	22	7	26.0	82932	37	
16		27	48.58	22	12	26.8	83026	37	1.16
17		27	41.16	22	17	38.4	83119	38	
18		27	29.89	22	23	0.6	83212	39	1.12
19		27	14.78	22	28	33.3	83304	39	
20		26	55.83	22	34	16.3	83397	40	1.08
21		26	33.06	22	40	9.4	83490	41	
22		26	6.48	22	46	12.2	83585	42	1.04
23		25	36.11	22	52	24.5	83681	42	
24		25	1.98	22	58	45.9	83779	43	1.00
25		24	24.12	23	5	16.0	83879	44	
26		23	42.54	23	11	54.3	83982	45	0.97
27		22	57.29	23	18	40.4	84088	45	
28		22	8.39	23	25	33.9	84198	46	0.94
29		21	15.89	23	32	34.1	84312	47	
30		20	19.83	23	39	40.6	84430	48	0.90
Dec. 1		19	20.27	23	46	52.7	84553	49	
2		18	17.25	23	54	9.9	84680	50	0.87
3		17	10.84	24	1	31.5	84813	51	
4		16	1.10	24	8	56.7	84952	52	0.84
5		14	48.11	24	16	24.9	85097	54	
6		13	31.96	24	23	55.2	85249	55	0.81
7		12	12.73	24	31	27.0	85408	56	
8	8	10	50.50	+24	38	59.5	9.85574	5 58	0.78

Astronomische Nachrichten, No. 4122.

Elliptic Elements and Ephemeris of Comet *c* 1906 (Kopff). The results given below represent the following observations made by Fath:

	1906 Gr. M. T.	Comet's Apparent α	Comet's Apparent δ
August	24.7063	$22^h\ 48^m\ 00^s.1$	$+10°\ 18'\ 21''.7$
September	5.6771	22 38 58.4	+ 9 28 57.9
	15.7371	22 32 26.9	+ 8 26 04.0

As indicated in bulletin No. 99 considerable variation in the elements based on a longer arc was anticipated. It therefore seemed advisable not to correct the first preliminary elements differentially, but to derive a close aproximation to the velocities and accelerations in α and δ for the middle date (September 5) from five observations as a basis for a direct computation of the orbit. For this purpose two other observations by Fath, made August 29 and September 8, were used in addition to those given above. The observations cannot be represented by a parabola.

The first decimal place of the period is determinate. Average errors of observation will affect the second decimal.

The residuals for the two intermediate observations and for a subsequent observation of September 19, made by Fath and kindly telegraphed by the Director of Lick Observatory are:

	O—C	cos δΔα	Δδ
August	29	+5″.4	+6″.2
September	8	+1.9	+4.1
September	19	+5.0	−5.9

ELEMENTS.

T=1906 May 2.0877 Gr. M. T.
Epoch=1906 September 5.67091 Gr. M. T.
M=18° 42′ 54″.6
ω= 18 28 44.9
Ω=263 45 23.6 } 1906.0
i= 8 44 09.8
log q=0.230114
log e=9.716356
log a=0.549258
μ=532″.255
Period=9.66633 years.

CONSTANTS FOR THE EQUATOR 1906.0.

x=r [9.994992] sin (13° 09′ 46″.8+v)
y=r [9.966482] sin (279 35 08.6 +v)
z=r [9.609834] sin (303 11 44.1 +v)

Parts of the check computation were performed by Mr. S. Einarson.

To facilitate interpolation the daily motions in α and δ, given in the columns headed α′ and δ′, have been computed by the formulæ published in *Popular Astronomy*, 13, 399, 1906. These data are not so essential in this case as they would be with more rapid and less uniform motion.

EPHEMERIS OF GREENWICH MEAN MIDNIGHT.

	True α			α′	True δ			δ′	log Δ	Br.
	h	m	s	s	°	′	″	″		
Sept. 23.5	22	28	47·5	−22·52	+7	31	46	−423.6	0.0764	0.65
25.5		28	05.5	19.36		17	41	421.0		
27.5		27	29.9	16.15	7	03	43	416.4	0.0894	
Sept. 29.5		27	00.9	12.91	6	49	56	409.8		
Oct. 1.5		26	38.3	9.66		36	25	401.5	0.1032	0.56
3.5			22.3	6.40		23	12	391.5		
5.5			12.9	−3.13	6	10	20	380.1	0.1176	
7.5			09.9	+0.13	5	57	53	367.4		
9.5			13.4	3.37		45	52	353.4	0.1325	0.47
11.5			23.4	6.58		34	20	338.3		
13.5		26	39.8	9.76		23	20	322.3	0.1478	
15.5		27	02.5	12.89		12	52	305.4		
17.5		27	31.4	15.97	5	02	58	287.8	0.1634	0.39
19.5		28	06.4	18.99	4	53	41	269.6		
21.5		28	47.3	21.93		45	01	250.8	0.1793	
23.5		29	34.1	24.79		36	59	231.6		
25.5		30	26.6	27.57		29	35	212.1	0.1952	0.32
27.5		31	24.5	30.26		22	50	192.5		
29.5		32	27.6	32.84		16	45	172.8	0.2112	
Oct. 31.5		33	35.8	35.32		11	19	153.3		
Nov. 2.5	22	34	48.8	+37.70	+4	06	32	−134.0	0.2271	0.27

Brightness August 24 = 1.00.

R. T. CRAWFORD,
A. J. CHAMPREUX.

LICK OBSERVATORY BULLETIN No. 100.

Elliptic Elements of Comet *e* 1906.—In *A. N.* 4123 Mr. M. Ebell gives elements of this comet determined from observations on the dates August 23, August 31 and September 12.

Epoch 1906 September 12.5 Berlin.

$M = 19°\ 43'\ 18''.8$			$\mu = 536.''204$
$\omega = 19\ \ 33\ \ 44\ .3$			$\log a = 0.547118$
$\Omega = 263\ \ 48\ \ 45\ .3$	}	1906.0	Period = 6.617 years
$i = \ \ 8\ \ 42\ \ 08\ .4$			T = 1906 May 3.09
$\phi = \ 31\ \ 18\ \ 13\ .3$			

An ephemeris is given extending to October 26, which agrees very closely with that from the elements obtained by Crawford and Champreux.

EPHEMERIS OF COMET *e* 1906.

[Computed from the Elements of Crawford and Champreux.]

Greenwich Midnight	R. A. h m s	Decl. ° ′	log *r*	log ρ	Brightness
Nov. 6	22 37 27	+3 58.6	0.3771	0.2430	0.25
10	40 24	3 53.5	.3811	.2587	.23
14	43 38	3 50.9	.3851	.2743	.21
18	47 06	3 50.8	.3890	.2896	.19
22	50 49	3 52.9	.3930	.3047	.17
26	54 44	3 57.4	.3968	.3196	.16
30	22 58 50	4 03.9	.4007	.3341	.15
Dec. 4	23 03 06	+4 12.5	0.4045	0.3483	0.13

Diagram of the Orbit of Comet *e* 1906.—The diagram given as frontispiece is drawn from the elements computed by Messrs. Crawford and Champreux, given in Lick Observatory Bulletin No. 100. The orbit is an ellipse having an aphelion just beyond the path of Jupiter and the perihelion outside the orbit of Mars. Its inclination to the plane of the ecliptic is only 8° 44′ so that its projection upon that plane is nearly of the same size and shape as the true orbit. The slow apparent motion of the comet is shown at a glance to be due to the relatively rapid movement of the Earth in the same direction around the Sun. It is evident from the diagram that the comet was discovered when at almost its nearest approach to the Earth and that since that time we have been moving away from it faster than it has receded from the Sun. It will probably become invisible with the largest telescopes within a month or two.

VARIABLE STARS.

Minima of Variable Stars of the Algol Type.

[Given to the nearest hour in Greenwich Mean Time beginning with noon. To reduce to Central Standard time subtract 6 hours, or for Eastern time subtract 5 hours.]

U Cephei		Z Persei		RT Persei		RT Persei		λ Tauri	
d	h	d	h	d	h	d	h	d	h
Dec. 2	17	Dec. 14	10	Dec. 1	12	Dec. 16	19	Dec. 4	21
5	5	17	11	2	8	17	15	8	20
7	17	20	12	3	5	18	12	12	19
10	5	23	14	4	1	19	8	16	17
12	17	26	15	4	22	20	5	20	16
15	5	29	16	5	18	21	1	24	15
17	16	Algol		6	14	21	21	28	14
20	4	Dec. 2	14	7	11	22	18		
22	16	5	11	8	7	23	14	R Canis Maj.	
25	4	8	8	9	4	24	11	Dec. 2	1
27	16	11	5	10	0	25	7	3	5
30	4	14	2	10	20	26	3	4	8
		16	23	11	17	27	0	5	11
Z Persei		19	19	12	13	27	20	6	14
Dec. 2	4	22	16	13	10	28	16	7	18
5	6	25	13	14	6	29	13	8	21
8	7	28	10	15	2	30	9	10	0
11	8	31	7	15	23	31	6	11	3

Minima of Variable Stars of the Algol Type.—Continued.

R Canis Maj.			V Puppis			RR Velorum			RR Draconis			W Delphini		
	d	h		d	h		d	h		d	h		d	h
Dec.	12	7	Dec.	9	22	Dec.	30	0	Dec.	11	2	Dec.	13	12
	13	10		11	9		31	21		13	22		18	8
	14	13		12	20	Z Draconis				16	18		23	3
	15	16		14	6	Dec.	2	1		19	14		27	22
	16	20		15	17		3	10		22	10	Y Cygni		
	17	23		17	4		4	19		25	6	Dec.	1	13
	19	2		18	15		6	3		28	2		3	0
	20	5		20	2		7	12		30	22		4	13
	21	9		21	13		8	20	WW Cygni				6	0
	22	12		23	0		10	5	Dec.	3	17		7	13
	23	15		24	11		11	13		7	1		8	23
	24	19		25	22		12	22		10	8		10	13
	25	22		27	9		14	7		13	16		11	23
	27	1		28	20		15	15		16	23		13	13
	28	4		30	6		17	0		20	7		4	23
	29	8		31	17		18	8		23	15		16	13
	30	11	S Cancri				19	17		26	22		17	23
	31	14	Dec.	6	5		21	1		30	6		19	12
Y Camelopardi				15	17		22	10	SW Cygni				20	23
Dec.	1	0		25	5		23	19	Dec.	4	2		22	12
	4	7	S Velorum				25	3		8	16		23	23
	7	15	Dec.	3	21		26	12		13	5		25	12
	10	22		19	19		27	20		17	19		26	23
	14	5		15	17		29	5		22	9		28	12
	17	13		21	16		30	14		26	23		29	23
	20	20		27	14		31	22		31	12		31	12
	24	3	RR Velorum			δ Libræ			VW Cygni			VV Cygni		
	27	11	Dec.	2	5	Dec.	3	8	Dec.	9	4	Dec.	1	19
	30	18		4	1		5	15		17	15		3	6
RR Puppis				5	22		7	23		26	1		4	18
Dec.	3	22		7	18		10	7	UW Cygni				6	5
	10	8		9	15		12	15	Dec.	2	4		7	17
	16	18		11	11		14	23		5	15		9	4
	23	5		13	8		17	7		9	2		10	16
	29	15		15	4		19	15		12	12		12	3
V Puppis				17	1		24	6		15	23		13	14
Dec.	1	4		18	21		26	14		19	10		15	2
	2	15		20	18		28	22		22	21		16	13
	4	2		22	14		31	6		26	8		18	1
	5	13		24	11	RR Draconis				29	19		19	12
	7	0		26	7	Dec.	2	15	W Delphini				21	0
	8	11		28	4		8	6	Dec.	3	22		23	23
										8	18	UZ Cygni		
												Dec.	14	8

Maxima of Variable Stars of Short Period not of the Algol Type.

The times of maxima only are given; the times of minima may be obtained by subtracting the interval printed in parentheses under the name of the star.

Y Aurigæ			T Monocerotis			ζ Geminorum			V Carinæ			T Velorum		
	d	h		d	h		d	h		d	h		d	h
(−0	18	)	(−7	22)		(−3	0)		Dec.	25	22	Dec.	29	8
Dec.	1	7	Dec.	3	5	Dec.	10	8	T Velorum			W Carinæ		
	5	4		30	5		20	12	(−1	10)		(−1	0)	
	9	1	W Geminorum				30	16	Dec.	1	12	Dec.	3	0
	12	21	(−2	16)		V Carinæ				6	3		7	9
	16	17	Dec.	2	16	(−2	4)			10	19		11	18
	20	14		10	10	Dec.	5	20		15	10		16	3
	24	11		18	4		12	13		20	1		20	12
	28	8		25	21		19	5		24	17		24	21

Maxima of Variable Stars of Short Period not of the Algol Type.
(Continued)

W Carniæ

d	h
Dec. 29	6

S Muscæ
(−3 11)

d	h
Dec. 4	16
14	8
23	23

T Crucis
(−2 2)

d	h
Dec. 1	4
7	21
14	15
21	9
28	2

R Crucis
(−1 10)

d	h
Dec. 4	13
10	9
16	5
22	1
27	20

S Crucis
(−1 12)

d	h
Dec. 5	2
9	18
14	11

S Crucis

d	h
Dec. 19	4
23	20
28	13

W Virginis
(−8 5)

d	h
Dec. 16	10
Jan. 2	16

V Centauri
(1− 11)

d	h
Dec. 2	6
7	18
13	5
18	17
24	5
29	17

R Triang. Austr.
(−1 0)

d	h
Dec. 1	19
5	5
8	14
11	23
15	9
18	18
22	3
25	13
28	22

S Triang. Austr.

d	h
(−2	2)
Dec. 1	8
7	16
13	23
20	7
26	15

S Normæ
(−4 10)

d	h
Dec. 10	8
20	2
29	20

κ Pavonis
(−4 7)

d	h
Dec. 2	19
11	22
21	0
30	2

X Cygni
(−6 19)

d	h
Dec. 3	0
19	9

T Vulpeculae
(−1 10)

d	h
Dec. 4	13
8	23

T Vulpeculæ

d	h
Dec. 13	10
17	20
22	6
26	17
31	3

WZ Cygni
Minimum
Period 14^h

d	h
Dec. 1	18
2	22
4	2
5	6
6	11
7	15
8	19
9	23
11	3
12	7
13	11
14	15
15	19
16	23
18	3
19	7
20	11

WZ Cygni

d	h
Dec. 21	15
22	19
23	23
25	3
26	7
27	12
28	16
29	20
31	0

TX Cygni
(−5 3)

d	h
Dec. 13	8
27	26

VY Cygni
(−2 2)

d	h
Dec. 4	10
12	7
20	3
27	24

δ Cephei
(−1 10)

d	h
Dec. 5	21
11	5
16	14
21	23
27	8

Approximate Magnitudes of Variable Stars on October 1, 1906, usually supplied by Harvard College Observatory has failed to arrive in time for this issue.

Elements of the Algol Variable 79.1906 Delphini.—In *A. N.* 4120 Mr. A. A. Nijland of the Utrecht Observatory gives the following elements of this variable.

Minimum = J. D. 2417424.53 (Gr. M. T.) + 4^d.601 E.

The range of magnitude is about 1.4 and the total duration of the minimum at least 10 hours. Minima occur at nearly the same hour every 23 days. Mr. Nijland gives predicted minima on November 15 at 8^h.5 and December 8 at 8^h.6, Greenwich time. Those two periods later than these would be better for American observers, i. e. November 24, 13^h and December 17, 13^h.

The position of the star for 1900 is

R. A. 20^h 38^m 52^s Decl. + 13° 35′.2

and its ordinary magnitude is 9.5.

GENERAL NOTES.

George E. Hale, Director of the Solar Observatory of the Carnegie Institution, located at Mount Wilson, California is to be absent on an eastern trip until the end of November. The great 100-inch reflector recently given to the Solar Observatory is the leading theme of interest to the astronomers on Mount Wilson.

Professor Asaph Hall, Sr., Norfolk, Conn., thinks the genesis of the solar system a vast subject. Not even LaPlace knew much about it, as he himself says.

Professor C. A. Young, Hanover, N.H., we are extremely sorry to learn is poorly in health. But his interest in Astronomy does not seem to lag. He has given some considerations to the "Planetesimal" hypothesis of the genesis of our solar system and he is disposed to regard it favorably though he has not given it careful study.

Detroit Observatory. Professor Hussey, Director of the Detroit Observatory, Ann Arbor, Mich., is now busy, going very largely into matters pertaining to repairs and reconstruction. He also has on hand the problem of mounting the new 36-inch reflector which is to be the next addition to the equipment of the Detroit Observatory. That observatory has a record in the past. Astronomy may expect a noble one for its future.

Professor Eric Doolittle of the Flower Observatory, Philadelphia, has kindly consented to send to this magazine soon an article on Double Stars. Professor Doolittle is giving considerable attention to this line of work, now, and that which he can give will be practical and useful.

Theory of the Kinetic Energy of Gases is claiming large attention among the physicists of the present time. Gustaf M. Westman has published part of an article on this theme in the October number of the *Journal of The Franklin Institute* of Philadelphia. In this part he discusses the relation between pressure and volume. He does this in two ways and then considers the energy derived when the pressure works against the atmosphere. In this work the mathematics used is not difficult, but interesting in its applications.

Thermodynamics of the Atmosphere. Professor Frank H. Bigelow gives his fourth article in the *Monthly Weather Review* on the numerical computations of the vertical ordinate, in which he considers the three theories regarding the formation of cyclones and anticyclones in the Earth's Atmosphere which may be referred to those authors who have been conspicuously associated with their mathematical development. They are (1) Forrel's cold center and warm center cyclones and anticyclones; (2) Oberbeck's symmetrical central cyclones and anticyclones, and (3) Bigelow's symmetrical cyclones and anticyclones. The mathematics and its applications furnishes some interesting reading for those who are practically concerned in meteorology or the student who cares to know about the applications of mathematics in working out physical theories that are now being studied.

G. W. Hough, Director of the Dearborn Observatory, Evanston, Ill., is keeping up observations on the planet Jupiter, and occultations of stars by the Moon. Nearly two years ago, he began to experiment on a new form of meridian mark for the purpose of ascertaining the azimuth of a meridian instrument. The work is now so nearly completed that we hope soon to have the privilege of giving it to our readers.

H. A. Howe, Director of Chamberlain Observatory University Park, Colorado, has been carrying the heavy burden of opening the University for the new school year, in the absence of the President who is now a candidate for Governor of the State. Have our readers noticed the places of responsibility that the

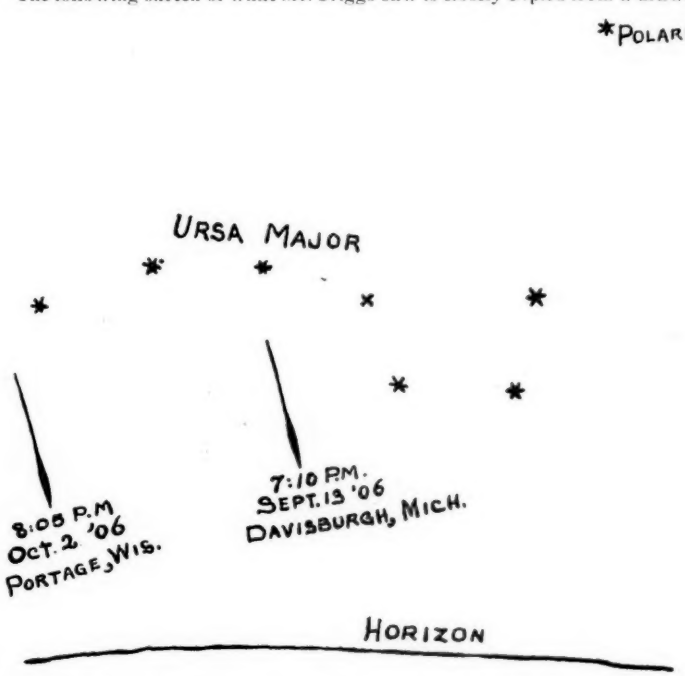
mathematicians and astronomers are holding in the colleges and universities of the United States?

George C. Comstock, Director of the Washburn Observatory of the University of Wisconsin, in answer to our request for his opinion of the Chamberlin-Moulton theory of the genesis of the solar system, says he has not gone minutely into the theory, but he has made some examination of it, and he regards it as containing very valuable material for the improvement of our theories of the cosmogony He understands that authors do not regard the theory as having taken its definite form, and that it stands now substantially, as a report of progress upon their problems. As such he thinks it is extremely interesting and valuable.

Sunspots are becoming few again. On October 16, for the first time in three years the Sun was observed at Goodsell Observatory to be entirely free from spots.

Very Brilliant Meteors. Mr. H. W. Griggs of Portage, Wisconsin, reports the appearance of two very brilliant meteors; one September 13, 1906 at $7^h\ 10^m$ p. m., at Davisburgh, Michigan, and the other October 2, 1906 at $8^h\ 05^m$ p.m. at Portage, Wisconsin.

The following sketch of what Mr. Griggs saw is closely copied from a drawing



made by himself accompanying his note showing the directions of the paths of the meteors and the bulging shape of them near the ends of the trails. It is an interesting fact that they so closely resemble each other.

Professor Simon Newcomb, Washington, D. C., upon request kindly gave us a few words on the Planetesimal theory of Chamberlin and Moulton. He says this subject is one lying so far outside of his sphere of work that he has not given any study to it. In scientific standing and ability, the authors are of the best, and what they say is worthy of serious consideration; but Professor Newcomb still retains a little incredulity as to our power in the present state of science to reach even a high degree of probability in cosmogony.

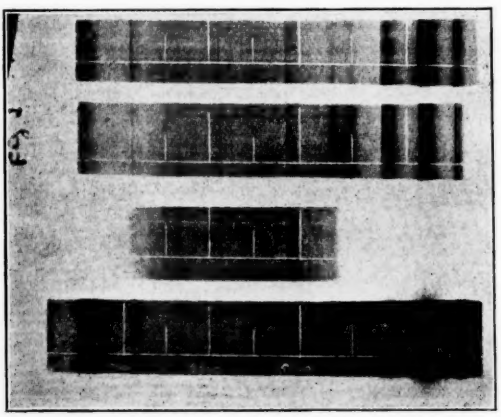
Joel H. Metcalf, Taunton, Mass., is doing considerable work in the discovery of asteroids. He says the United States Naval Observatory is at present taking them off his hands. Thinking that the astronomers at Washington must be getting their hands full of this kind of work, he asks if Goodsell Observatory would not like to follow some of his new asteroids with its 16-inch refractor, and compute their orbits from the places so found. It is probable that our Observatory will soon take up that work for the practice that our advanced students in practical astronomy need.

Queries. 1. About the end of the 17th Century Chesaux, a speculative writer, asserted that for an infinite universe the heavens would be equally luminous everywhere, and the illumination would be the same as sunlight seen from the Earth. What are the arguments for this conclusion if it is true? H.G.

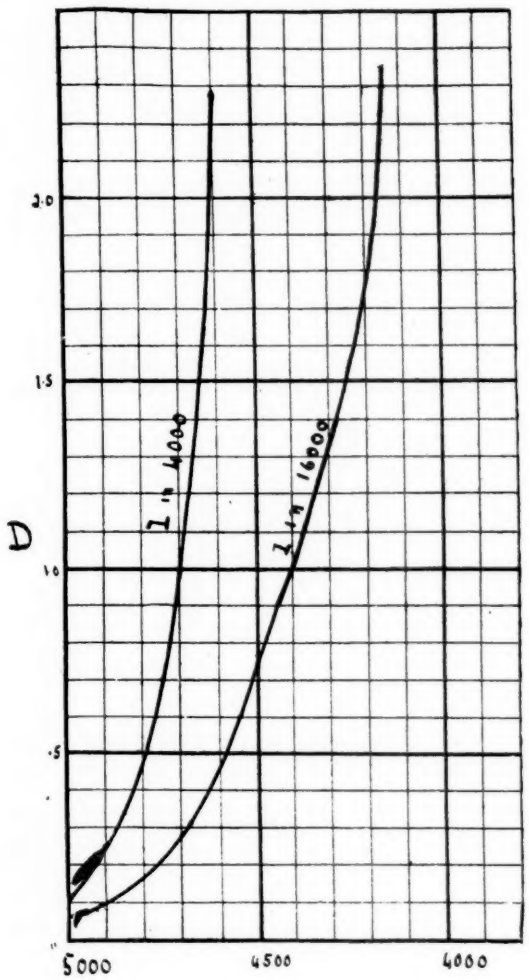
2. Do astronomers at present incline to the belief that the universe is infinite in extent or limited? P. W.

3. Is there any evidence that light passing through interstellar spaces before it reaches the Earth's atmosphere loses any of its brightness?

Filter Yellow K. The introduction of a new yellow dye by Meister, Lucius and Brüning, is an event worthy of notice. The yellow dyes at present in existence are mostly unsatisfactory, either in permanence or insolubility, the best being Flavazine T., or Tartrazine, which, however, when used in dilute solution, permits the extreme ultra-violet to pass. There are two dyes at present



which are greatly needed, the one a dye with absorption similar to tartrazine, i. e., gradual absorption but which cuts out the whole of the ultra-violet; the



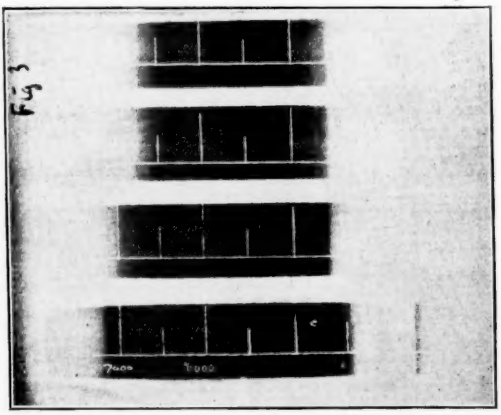
[Fig 2]

other, a dye with exceedingly sharp absorption, so that in very dilute solution it will cut out the ultra-violet without appreciably affecting the visible violet—a dye with an absorption like potassium bichromate solution, for example. Filter yellow K perfectly corresponds to the first of these dyes.

The curves shown in figure two correspond to solutions of strength 1-4,000 and 1-16,000 c.m. It is easily seen from these that the absorption is gradual,

and suitable for orthochromatic screens. Figure one shows the spectrum of the enclosed arc photographed

(a) Without a filter;
(b) through filter yellow K. 1:4,000;
(c) through filter yellow K. 1:16,000;
(d) through tartrazine of such strength that visually it is identical with (b). Figure three shows the absorption of 1:4,000 filter yellow K., with differing



exposures. The dye is very easily soluble in water, and mixes well with gelatine. As regards permanence, tests on this point have proved satisfactory.

C. E. Kenith Mees.
S. H. Wratten.

British Journal of Photography, Sept. 28, 1906.

New York Academy of Sciences Exhibition. We are just in receipt here at Goodsell Observatory of a letter from Charles Lane Poor which will be of general interest to our readers as outlining in brief the exhibition now being planned by the New York Academy of Sciences. The text of the letter is given herewith.

"On December 27th and 28th next, the New York Academy of Sciences will hold an exhibition illustrating the recent advances in the different departments of the science. The exhibition is to be held in the American Museum of Natural History, in connection with the meetings of the American Association for the Advancement of Science, the Astronomical and Astrophysical Society and other kindred organizations. It is many years since such an exhibition has been held in New York, and it is hoped that it can be made interesting and instructive.

As Vice-Chairman in charge of the Division of Astronomy, I should esteem it a favor if you would coöperate with us to make the exhibition a success by sending examples of the work of your observatory. Such exhibit might consist of new and novel apparatus, attachments to telescopes, photographs, transparencies, charts, diagrams, or other material which will best illustrate the work that has been done in recent years, or work that is now in progress.

If you can coöperate with us in this matter, I would like to hear from you early in October, as to the general character of your exhibit, the amount and character of the space you would require (vertical and horizontal areas) and whether any special equipment would have to be prepared by the Museum.

Any apparatus or material sent in will be most carefully cared for and arranged under my direction by the staff of the American Museum.

CHAS. LANE POOR, Vice Chairman."

The British Astronomical Association. We have just received a list of the members of the British Astronomical Association which is brought up to September of this year. This list contains nearly one thousand names, and is alphabetical, and in other regards in very useful form. If the work of this list is as accurately done as it appears to be somebody should have a good deal of credit for it.

This association has three branches in addition to the parent body. They are the West Scotland Branch, the New South Wales Branch and the Victoria Branch. The names of the members of all the Branches are given in this general list but they are all designated by symbols so that the name of every one may be easily classified. The three columns of each page show the date of election, the full name and the full address. Names of the original members are without date, that means members who joined the Association prior to December 31, 1890.

We have also been impressed favorably with the way the Association is organized. It is divided into sections for the purpose of carrying on astronomical work. There is the Mars section, the Solar section, Colored Star section, Mercury and Venus section, Lunar section, Variable Star section, Cometary section, Jupiter section, Double Star section and Meteoric and Photographic sections. These ten sections give a wide range of work and from the reports made by the directors of the sections, very much of interest to all lovers of astronomy is gathered and put in useful form for records. Most of the prominent English astronomers are members of this Association and their influence and help give wise guidance to this large body of interested scholars.

This thing is just what the United States lacks. There is not here that helpful relation between the amateur and the practical astronomer. It would be a great advantage to the science if there were something like it on this side of the ocean. The felt need of it is manifest in the number of names from America that we see in this list.

Celestial Photography. In his presidential address to the British Association at York, Professor E. Ray Lankester stated that the invention of the dry plate, which has made it possible to apply photography freely to astronomical work, is the chief cause of the great expansion of astronomy since 1881. It was the dry plate which made long exposures possible, and thus enabled astronomers to obtain regular records of faintly luminous objects, such as nebulæ and star-spectra. Roughly speaking, those visible to the naked eye may be stated as eight thousand; this is raised by the use of our best telescopes to a hundred million. But the number which can be photographed is indefinite, and depends on length of exposure; a thousand million can certainly be so recorded. By the photographic method hundreds of new variable stars and other interesting objects have been discovered. New planets have been detected by the hundred. Up to 1881 two hundred and twenty were known. Up to 1881 only one was found; namely, Stephania, being No. 220, discovered on May 19. Now a score at least are discovered every year. More than five hundred are now known. One of

these—Eros (No. 433)—is particularly interesting, since it is nearer to the Sun than is Mars, and gives a splendid opportunity for fixing with increased accuracy the Sun's distance from the Earth. Two new satellites to Saturn and two to Jupiter have been discovered by photography (besides one to Jupiter in 1892 by the visual telescope of the Lick Observatory). One of the new satellites of Saturn goes 'round that planet the wrong way,' thus calling for a fundamental revision of our ideas of the origin of the solar system. The introduction of photography has made an immense difference in spectroscopic work. The spectra of stars have been readily mapped out and classified, and now the motions in the line of sight of faint stars can be determined. This "motion in the line of sight," which was discernible but scarcely measurable with accuracy before, now provides one of the most refined methods in astronomy for ascertaining the dimensions and motions of the universe. It gives us velocities in miles per second instead of in an angular unit to be interpreted by a very imperfect knowledge of the star's distance. The method was in 1881 a mere curiosity, which Huggins was almost alone in having examined, though visual measures had been begun at Greenwich in 1875, and were continued for many years, only to be ultimately found to be affected by systematic error. The photographic method started by Vogel in 1887 really has made all the difference, and this work is now a vast department of astronomical industry. Among other by-products of the method are the "spectroscopic doubles," stars which we know to be double, and of which we can determine the period of revolution, though we can not separate them visually by the greatest telescope. Work on the Sun has been entirely revolutionized by the use of photography. The last decade has seen the invention of the spectro-heliograph—which simply means that astronomers can now study in detail portions of the Sun of which they could previously only get a bare indication.

PHOTO-BEACON, October, 1906.

A Road to Mt. Wilson. On the occasion of President Woodward's recent visit to the Solar Observatory, it was decided to widen the "New Trail," which ascends from the foot of Eaton Cañon, into a road suitable for the transportation of the heavy castings of the five-foot reflector mounting. President Woodward recommended that Mr. Godfrey Sykes, superintendent of construction at the Desert Botanical Laboratory of the Carnegie Institution, be placed in immediate charge of the work, on account of his skill as an engineer and his experience in the building of trails in Arizona. This was rendered possible through the courtesy of Director MacDougal, of the Botanical Laboratory, who arranged to give Mr. Sykes a leave of absence. Work has been commenced, and will be pushed forward as rapidly as possible, in the hope that the structural steel for the building which is to contain the five-foot reflector may be sent up the mountain in May, 1907.

The width of the road, which is determined by the size of the large castings to be transported, will average about eight feet, widening to ten feet or more on the turns. The automobile truck, on which the materials for the building and telescope will be carried, has been ordered from the Couple-Gear Freight-Wheel Company, of Grand Rapids. It has been specially designed for the purpose, as the ten-foot worm-gear and other large and cumberous parts of the mounting (some of them weighing five tons each) could not be carried on the standard truck. The motive power will be a gasolene engine of about forty horse-power, driving a large direct-current dynamo. The dynamo will supply current to four motors mounted with the four wheels of the truck. This truck, which is capable of turning within a very small circle, has been tested by Professor Ritchey and

found to be remarkably well adapted for our purpose. It can be steered equally well from either end, thus permitting it to be reversed, if necessary, at sharp re-entrant turns.

As soon as completed, the truck will be used in the construction work on the trail. For this purpose it will carry an air-compressor of sufficient capacity to supply several rock-drills. The compressor will be driven by the gasolene engine of the truck, and will greatly reduce the cost of the rockwork.

GEORGE E. HALE.

PUBLICATIONS OF THE ASTRONOMICAL SOCIETY OF THE PACIFIC.
August 10, 1906. No. 109.

Graft Disturbing Peaceful Astronomers. Quite a number of years ago, a man in Washington who was in the employ of the Government was successful in inventing an attachment for a clock which would enable a telegraph operator to correct or set a clock to true time at any distance. By the one who made it the invention was thought to be valuable. The inventor, we are informed, sold it to two companies for the sum of $30,000. One of these companies made clocks, the other was a telegraph company operating largely on railroad lines. The latter company at once began to rent the clocks from the other company to railroads, dividing between them the income derived from these rentals. The effect of this arrangement was to supplant very generally the time-service which had been given to the railroads from astronomical observatories in different parts of the United States.

Part of this tripartate agreement involving the sale of the clock patent was that the Washington time should be given to correct these railway clocks daily from the United States Naval Observatory by means of the Western Union Telegraph Company. In this way local observatories all over the United States have been deprived of the local patronage of the railroads for time which was formerly used and the work of science in astronomical lines. It is a matter of profound regret that such a commercial arrangement should ever have taken place to destroy the work of useful science.

But this is not the worst of it; the graft part of this greedy commercialism is beginning to show itself in a way that will be a disturbing element probably very soon. Unprincipled men who have money and own stock largely in these aforenamed companies are now making it necessary that railroad employees use inferior grades of watches, at high prices that they may realize out of the trade what can not be obtained legitimately in rightful competition. This is nothing less than graft pure and simple. Possibly Theodore Roosevelt, one of the noblest men that ever sat in the presidential chair, may be able to strike this graft squarely in the face, as he has done, and is doing most rigorously in other directions. The common people of this great nation are enthusiastically proud of him, and they have a right to be.

Length of the Radius of the Earth. We have not the means of verifying the statement that Dionysodorus (B.C. 50) determined the length of the radius of the Earth, approximately as 42,000 stadio, which, if taken as an Olympic stadium (202¼ yds.), is a little less that 5000 miles. Anstosthenes's result, found about 200 B.C. if calculated by the same stadium was 4600 miles. We know the method used by the latter astronomer, but not that of the former. It would seem that some history must explain the method of Dionysodorus, so the value of his stadium might be known. That would be useful information.

Publications of the U. S. Naval Observatory. We have received Vol. 4, pt. 4 of the publications of the U. S. Naval Observatory. This volume contains splendid accounts of the total solar eclipses of 1900 and 1901, Transit circle reduction tables, Equatorial reduction tables and Standard time. The photographic work is large in amount and excellent in result. The illustration of the shadow bands is novel indeed.

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